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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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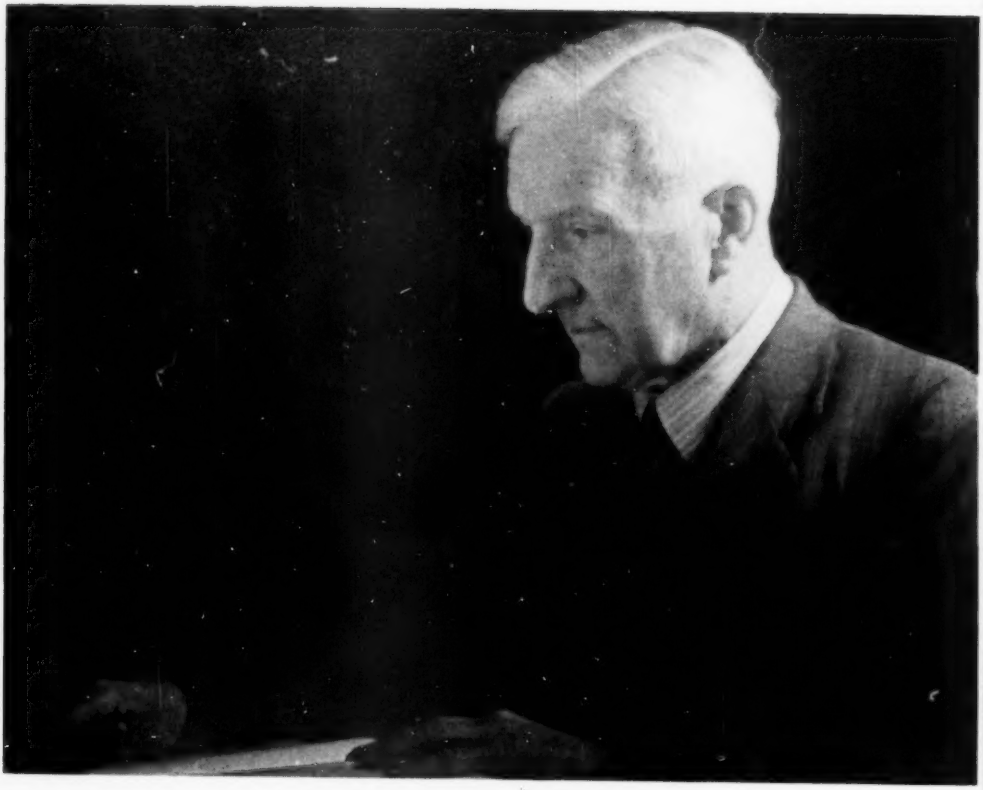

G. N. M. TYRRELL

(See pages 24-33)

The Journal *of*
Parapsychology

Volume 17 MARCH, 1953 Number 1

EDITORIAL

PARAPSYCHOLOGY AND ITS PERSONNEL

ARE NEW RECRUITS joining the forces at work in parapsychology in numbers that make up for the losses? Such a question must arise in many minds with the news of the loss of Mr. J. G. Piddington and Mr. G. N. M. Tyrrell in recent months. No doubt there are many who want to know, likewise, whether a quality and calibre of research worker are being maintained comparable to those of the distinguished pioneers responsible for the beginnings of our science.

Such questions are largely rhetorical and probably no one would expect a very clear-cut answer. As for quality and calibre, it is, of course, what one accomplishes in parapsychology that must be considered, not how distinguished he may have been in some other field. Those who are new in the field today may require a lifetime to reach the full stature of their achievement. How then can one compare them? The same uncertainty attaches to whether or not they are only transient workers in parapsychology or permanent contributors, devoted to the subject for a lifetime.

Some perspective on the personnel at work in a field may, however, be gained from a few more or less reliable general impressions, even though these are subject of course to our own limitations of observation and viewpoint. Over the period of the last twenty years there have been hundreds of people, perhaps the majority of them

students in the universities, who have taken something of a fling at research in parapsychology. Most of their efforts have been devoted to testing ESP or PK, some of them in connection with classroom assignments, some of them arising out of private interests only.

Not many explorers of this type have followed through to any continued activity in the field. That is understandable, for their first experimental adventure showed them how slow and laborious such work can be. The emphasis upon precautionary requirements and the need for careful checking and statistical analysis have robbed experimental projects of much of their dramatic attractiveness. It is not likely in such cases that there was a very profound appreciation of the significance of the problem or of any possible findings. It is doubtless best for all concerned, certainly for the individual himself, that such an inquirer drop research in the subject after completing his first assignment.

Like most university research, some of the work of the early years of the Parapsychology Laboratory at Duke University was tied up with graduate study, mostly in the Department of Psychology, of which the Laboratory was originally a part. During that period there were a score or more graduate students who played an active part in the research work of the Laboratory at the same time that they were pursuing their graduate studies. Some of them, under the guidance of the permanent staff of the Laboratory, did research on psi problems for their graduate theses. As time passed, however, it became evident that this way of getting research assistance made undue demands upon the Laboratory staff, and so, as the problems became more technical and required full-time as against part-time attention, the graduate student assistantships were abandoned.

The effort at giving graduate instruction in parapsychology was likewise found to be a burden on the much needed research time of the staff as, indeed, was the whole arrangement of participation in the teaching activities of the Department of Psychology. With the separation of Laboratory and Department came the discontinuance of this cooperative plan which had brought young people, at least for a time, into research activity in parapsychology as part of their graduate training. It was a loss of one source of young research

personnel, but on the whole the gain in time and freedom of operation for the regular staff has seemed far to outweigh the loss.

Taking a wider view of the situation, we have come into contact with numbers of people located in various parts of the world who have been devoting themselves to parapsychology as an avocation. They have been doing it often under great difficulties and usually in comparative isolation. These are individuals who have at least demonstrated their persistent and devoted interest in the problems of the field. Closer contact between the Laboratory and these isolated individuals has, through the years, proved to be very rewarding to the laboratory workers. Actual geographic separation is becoming less and less of a crippling handicap. Thanks, too, to generous provisions from various sources, it has been possible to bring some of these workers together for periods of joint research and exchange of experimental results and ideas. The maintenance of such cooperation among those who are already actively working seems beyond question to be the best expenditure of time and funds toward helping to keep an active research personnel in the field today.

It is important, of course, to provide opportunity for interest in the subject to develop freely in individuals, especially in the collegiate communities where many of the abler young people are naturally concentrated. For this purpose active research centers are needed in such communities and in other locations where beginners may undertake exploratory work in the field and where visiting research workers may gather. But beyond the provision of this free opportunity and the necessary facilities, it seems at this point in our experience better that there should be no complicating inducement, no stimulation of secondary or ulterior motives, so that those who do venture to do research in psi will be more likely to continue because of their own basic interest.

Up until recent years, there has been enough for the beginner to do that gave some prospect of success to justify broad reliance upon a more casual and perhaps recognizedly temporary interest in the field. Undoubtedly, the great amount of help obtained from temporary workers during this period has added up to a large profit for parapsychology; but now it seems doubtful whether for the future such is likely to be the case.

It is hard to say whether continuing effort should be made to preserve the semi-amateur status of psi investigation for an indefinite period ahead; or, on the other hand, whether it would be better to endeavor to make the research more entirely professional, to push on with all concentration possible toward making parapsychology a technically sophisticated science, integrating its findings as far as possible, not only within its own boundaries but also with the body of related knowledge from other sciences.

A third alternative may suit the present stage of parapsychology better than either of these two extremes alone. That is to encourage a broader conception of the research field combining the advantages of the other two alternatives. A certain amount of technical advance and general systematic development of the field is necessary for progress; indeed, it is highly desirable. But there is much exploration yet to be done by comparatively inexperienced workers in parapsychology who happen to be strategically situated or who may be gifted in some particular way that affords unusual advantage. The research field needs the broadest possible and the most widely open frontiers, leaving to the later devices of experimental verification the steps of final careful screening for scientific approval. The fact is that there is in these psi researches more than ever a very real and urgent need for the most alert attention to every possible scrap of relevant information or experience that bears on the problem. It is necessary now to reach out further into the spontaneous and, in general, the nonexperimental and nonmeasurable occurrences. Like any research field, the greatest need is for new insight, better ideas.

All this reflection argues against allowing the conception to develop that a research worker in parapsychology is necessarily one who experiments in a laboratory, or uses a special set of techniques or terms. He may be a minister in Nevada who is making a study of the unusual prophetic experiences of one of his parishioners, a physician in India who is scanning the folkways of the local populations for manifestations that challenge accepted principles of explanation, a psychologist on an expedition to study the Australian aborigines who extends his battery of tests to include psi phenomena, a psychiatrist in Switzerland who is trying to find out something about the effective correlates of the telepathic experiences of his

patients, or a mother in California who is a puzzled and anxious but a careful and systematic observer of her five-year-old daughter's flashes of ESP.

It would be a serious mistake for those who are working on psi problems in one area or by one standard to get out of touch with all these others. It would in fact be hard to say from what direction the greatest contributions are most likely to come.

Therefore, there is greater need than ever for a widely extended research personnel in the field and in the laboratory. The cultivation of intelligent interest in the problems, the development of standards of inquiry and reporting, and the ready exchange of help of various kinds—of information, of transportation, of conferences, and of facilities for publication of reports—all should help to encourage the bringing in and bringing together of those from whom the broad category of research workers of the future in parapsychology will be drawn. And this is the nub of the personnel problem. It requires a broad and active educational program, to which the societies for psychical research have made the principal contribution through the years, to keep interest in the psi problem area alive in the world and to assist in the cooperation that is needed to bring isolated facts and isolated workers together as far and as well as may be.

From the beginners, the amateurs, the isolated workers, from whatever professional or nonprofessional background they may come, there will emerge here and there an individual who will, by his own experience, effort, and adaptation, develop into a more or less professional researcher in parapsychology. Some of these will find their way, at least for a time, to such research centers and laboratories as can be maintained; but this is not to say that those who do not do so are playing a less important role. Such matters are too relative, too individual, and too uncertain for any valid over-all judgment. But by present signs there are likely to be many of these varied individuals to replace the losses of the past, so that one need not feel anxiety about the quality and calibre of those who can succeed in making their contribution; the requirements have not become easier, to say the least, nor the standards lower.

J. B. R.

PSYCHICAL RESEARCH PAST AND PRESENT

THE ELEVENTH FREDERIC W. H. MYERS MEMORIAL LECTURE[1]

By Robert H. Thouless

The Seventieth Anniversary of our Society seems to be a suitable occasion for taking stock of our position, for considering how far the Society has succeeded in achieving the aims of its founders, how much still remains to be done, and whether we are now moving in the right direction to achieve those aims more fully. If one of our founder members (let us say F. W. H. Myers in whose memory these lectures are given) were with us today, would he feel that something had been accomplished during the years since he left us, and would he feel reasonably satisfied with what we are trying to do now?

It is right that we should honor our forebears but not by maintaining forever the positions which they held. We honor them best by going further along the paths which they began. If we were still doubtful of all the things that our founders were doubtful about, the Society might as well have never been started. We are not, for example, under the necessity of proving again the reality of paranormal cognition. That has been done, and a skepticism about such things which was reasonable in 1882 is no longer reasonable in 1952.

One development in which our founders might properly take a great deal of satisfaction is the much wider interest in the subject of psychical research as shown by the founding of societies for psychical research in many countries and the opening of facilities for psychical research in several universities, and most particularly in the opening, twenty-five years ago, of the Parapsychology Laboratory at Duke University, North Carolina, under the able and vigorous leadership of Professor Rhine. When it started in 1882, our Society made a lonely protest against the indifference of the learned world to a sys-

[1] Delivered on June 4, 1952, to the Society for Psychical Research in London. Reprinted here by permission of the Society.

tem of odd and unexpected phenomena which they felt it was their duty either to understand or to prove to be baseless fabrications. We are no longer alone, and I think we may reasonably regard the successes of these other bodies and institutions as in part a fruit of the work which our founders started when they created the Society for Psychical Research.

If we ask ourselves what field of activity in psychical research we could point out to our founders with most satisfaction as that in which the greatest advances have been made in recent times, I think we should be inclined to choose the field of experimental work on psi capacities. It is true that much of this work has been done in America, but in this country also we have Soal, Tyrrell, and Whately Carington who have all made notable contributions. It is not my intention this evening to say much about this work since I have already discussed it at length in a discourse I gave before the Royal Institution the winter before last which has recently been reprinted in the JOURNAL OF PARAPSYCHOLOGY (**18**).

Much that is new has been found out but we are, I think, far from having exhausted the possibilities of parapsychological experimentation. As has been pointed out by Mrs. Rhine (**12**), we find the challenging situation that, whereas in spontaneous cases of psi experience there is reported not only a coincidence between the event and the percipient's reaction to it such as we find in our experimental work, but also very commonly an awareness on the part of the percipient of the connection between his reaction and the event such as we have not succeeded in getting in our experimental work (except possibly in very rare cases). Our intention in experimental work is to reproduce under the strictly controlled conditions of the laboratory the same phenomena as those which occur sporadically in the world outside under less favorable conditions for exact study, and we cannot be content until all the well attested spontaneous facts can be reproduced experimentally, the subject's knowledge as well as the mere behavioral fact of a right response.

Then there is also the problem of intra-personal factors in psi responses to which Professor Gardner Murphy drew our attention in his Presidential Address (**9**). This invites investigation of psi effects in small groups by the methods of experimental social psy-

chology developed by Sherif, Lewin, and others. This is a field still unexplored which might offer a rich harvest to an investigator trained in the application of these experimental methods.

The complaint is sometimes made against the Society that we are so confined to a narrow set of interests that we are not concerned with the really important things that are happening in the world around us—with, for example, the "telepathy" performances of the Piddingtons. In a recent book (**4**) the S.P.R. are accused of having made no direct offer to the Piddingtons to investigate their performance. There was, however, an indirect offer reported in our *Journal* (July-August 1949) which it is said that the Piddingtons declined to accept. If the Piddingtons had accepted this offer and had proved to have psi capacities of the order suggested by their stage performance, they would, of course, have been of great interest to the Society as potential subjects for experimentation. But the interest of the Society would necessarily have been of a different kind from that of the general public. The general public seemed to be divided into two groups: those who believed that the Piddingtons were using telepathy and that their performance gave proof of the reality of telepathy, and those who believed that there was no such thing as telepathy and that the Piddingtons were using trickery of some kind or another. Obviously a well-informed member of this Society could endorse neither of these opinions. We know that the reality of telepathy (in the widest sense of psi capacity) has been proved by a series of carefully controlled and well devised experiments, and that this reality depends in no way on whether the Piddingtons were using telepathy in their performance. We know also that under the conditions of a stage performance we cannot get convincing evidence as to whether telepathy is being used. Even if the Piddingtons could have reproduced their performance under strictly controlled conditions, that fact would not have added appreciably to the weight of evidence we already have.

I gather that Mr. Piddington realized that to give his performance maximum entertainment value it was necessary to keep the public guessing whether it was done by telepathy or not, so he made no definite claim to be using telepathy but told the audience that they

must be the judge. It would perhaps not have contributed to the entertainment value if he had also mentioned that under the conditions of a stage demonstration the audience could not have the control that would be necessary before they would be justified in coming to any certain conclusion. It would be churlish to complain of a brilliantly successful entertainment that it contributed nothing to our scientific knowledge of psi phenomena, but it would be foolish to imagine that it did make such a contribution.

If we can feel reasonable satisfaction at progress in the experimental field, there are other directions in which little or no recent progress can be reported. Our knowledge, for example, of hauntings and poltergeists seems to be much as it was about the end of the last century. The best study of an apparitional haunting is still the report of the "Morton" ghost observed during the first seven years after the founding of this Society (**8**) while the most recent well-observed account of a poltergeist is still, I think, Barrett's study of the haunting of Derrygonelly Farm reported in 1877 (**2, 3**).

There are, of course, reasons for the relatively slow development along these lines. Not, I think, primarily if at all increased scarcity of cases. I have the impression that reports of apparitional hauntings may be somewhat less frequent than they used to be but reports of poltergeists, although by no means common, seem to be no rarer than they were. The psychical researcher is handicapped by the fact that the average person living in a haunted house generally looks at the haunting in much the same way as he would an invasion of his larder by black beetles: not as an interesting and rare occurrence to be carefully studied but as an unbearable nuisance to be got rid of as quickly as possible. He is more likely to send for an exorcist than for a psychical researcher.

We have also the difficulty that many of the cases of alleged hauntings reported to the Society are of a trivial nature and may possibly be explained as due to misunderstanding of entirely normal events. But if a good haunting were presented to us, our resources for investigation are quite inadequate. In order to have a really illuminating investigation which could give us some understanding of the laws of hauntings, more is required than a visit by a senior

member of the Society or even by the Research Officer. A prolonged occupation of the house by a team of research workers with full equipment of recording devices would be necessary, and few householders would welcome that. It is not often that a psychical researcher has the advantage that Miss Morton had of living in a haunted house, and it was this fact that gave Miss Morton's account its preëminent position. If a real attack on this problem were to be made, it would be necessary that the Society should be able to buy haunted houses and to put a properly equipped research team into them. Until we are in a position to do that it is unlikely that the study of such problems will advance far beyond where it was in the early days of the Society.

Another problem which occupied our founders soon after the beginning of our Society was the question of the obtaining of convincing evidence on the problem of survival by the human personality after bodily death. Have we advanced in the direction of obtaining an answer to this problem? Not, I think, as far as they might have hoped.

At the present time we are often discouraged from attempting experimental investigation of survival by those philosophers who tell us that the statement that the human spirit survives bodily death is a meaningless one, incapable of empirical verification, and only seeming plausible because of the misuse of the word "spirit" as if it stood for an entity which could exist apart from the body. These are two separate points. The first seems to me to be clearly erroneous. The survival of the human spirit is certainly verifiable in principle even though it may be contended that it has never been verified in fact, and verifiability in principle is all that is required to make a proposition meaningful. It would be verifiable in principle if any evidence that would convince us of the survival of the same personality from an earlier to a later point of time while he was still alive, could also be produced for two points of time at the first of which he was alive and at the second of which he was dead. No such evidence may ever have been produced but it is not unimaginable and therefore the proposition that so-and-so has survived death, although it may be false, cannot be held to be meaningless.

The second point in this objection to regarding the problem of survival as one for empirical investigation is that the expectation of survival is said to be based on a misuse of language to imply that "mind" or "spirit" are words standing for some entity other than the physical body and its life. As an argument against the possibility of survival, this seems to me to be based on a failure to understand the limitations of linguistic analysis. This is a valuable instrument for studying how far our use of language is adequate to express the facts; it cannot be used as a means of finding out what are the facts. We can, of course, adopt a terminology which excludes such terms as "mind" or "spirit" on the ground that these are not necessary for explaining the ordinary facts of normal psychology, but the empirical question remains as to whether or not we go on existing after bodily death. An affirmative answer is not made probable by the fact that we can use a language which implies the possibility of survival, but neither is a negative answer made necessary by our choice of a language which implies that there is nothing to survive. We must try first to discover the facts, and then make any changes in our language necessitated by the facts. If survival is a fact, some such noun as "mind," "soul," or "spirit" for what survives becomes necessary. To decide a question of fact we must look at the facts, not at our language.

Have we, then, succeeded in demonstrating the fact of survival? There are two questions we might ask about this which it is important that we should keep distinct. First, the question as to what is a reasonable sort of judgment to make, taking all proper grounds of belief into account. Secondly, the question as to whether we can produce coercive evidence such as should convince any reasonable person. It is important to be clear as to which question we are asking since the answers may well not be the same. I think that some of the suspicion that psychical researchers are obstinately negative is a result of the fact that they are supposed to be answering the first question when they are really trying to answer the second. It is also, I think, arguable that the suspicion is not altogether unjustified because some psychical researchers have not always clearly distinguished the two questions and have talked as if, in answering the second question, they have also answered the first.

It is the second question with which we are here concerned. I think the answer to this question as to whether we have obtained coercive evidence of the reality of the survival of death must still be "No." I do not, however, wish to be misunderstood as suggesting that we must also give a negative answer to the other question as to whether there can be rational grounds for believing in survival. It is reasonable for a man to believe in the survival of the human spirit because this is part of the teaching of a religious faith which he accepts. He may also have reasonable grounds not only for believing in survival but also for believing that the surviving spirits of the departed can communicate through mediums. He may, for example, be convinced that information has been given to him in a séance which could only have been known to the dead person who is the ostensible communicator, or he may feel an overwhelming conviction that his communications so completely express the personality of the ostensible communicator that he finds it impossible to doubt that he is really in communication with that personality.

These may be sensible reasons for belief but they fall short of being coercive proofs of the survival hypothesis. The evidential value of information given in séances is difficult to assess, and it may appear much greater to the recipient than it does to impartial investigation. There is always the possibility of mere coincidence even apart from the further difficulty of knowing what are the limits of the psi capacities of the medium or of the medium-sitter combination. Impressions of the real presence of a previously known personality are even more subjective and resistant to exact evidential assessment. I can well believe that this is an experience impressive to those who have had it but of litle value as evidence for the conviction of others. I have never had it myself. When I have communicated through mediums with those who claimed to be my departed friends and relatives, they have sometimes spoken as I should have expected, more often in a way quite different, and I have been more inclined to reflect that if they were the spirits of those I knew on earth their personalities must have been changed considerably by the situation of being in another world. That, of course, is no argument against their identity; it is the type of situation which might be expected to

make radical changes in one's way of thinking and talking. It does, however, make identification by quality of personality difficult.

That we should go on asking for proof of the identity of communicators seems to many people to be one more indication of the obstinate skepticism of members of the S.P.R. But it is a real problem, and it is also a difficult one if we want adequate proof and not merely sufficient grounds for a probable opinion. Even if we believe that some ostensible communicators are the departed spirits they claim to be, the question arises again in connection with every new communicator.

It is sometimes asked whether we are equally skeptical in everyday life; whether, for example, the psychical researcher who refers to "ostensible communicators" is doubtful about the identity of every person who rings him up on the telephone, and whether he tells his wife that there has been a call from "ostensibly Mrs. Y." Obviously he does not, but I think he certainly would do so if he found that he was receiving messages on the telephone as doubtful in their origin as those he often has in mediumistic séances.

There is plenty of reason for feeling doubtful whether mediumistic communications are always coming from those from whom they are purporting to come. For example, Mr. Alfred Dodd obtained through Mrs. Dowden scripts which were ostensibly from Francis Bacon, in which Bacon claimed to have written the plays and sonnets commonly attributed to Shakespeare (7). Later Mr. Percy Allen, who upheld the theory that Lord Oxford wrote Shakespeare's plays, had communications ostensibly from Francis Bacon through the same medium (1). In these scripts, Bacon said that he had written neither poetry nor plays with the exception of one short passage in the composite play *Sir Thomas More*. He was naturally asked why he had given a different reply to the earlier sitter, and answered that these messages had not come from him but from a deputy who had never been personally in touch with him and was mistaken as to the facts. It seems clear that the communicator could not have been Bacon in both cases, and it is also obvious that, if the possibility of impersonation or other source of mistake as to identity is admitted, it may not have been Bacon in either case.

Moreover, we are liable to find, in addition to communications which are plausible products of the persons from whom they are supposed to come, other communications which are by no means plausible. Communicators who are ostensibly people who in this world have been eminent physicists may babble about waves, vibrations, and velocities like uninstructed children; eminent literary men who, on this side of the grave, were forcible and original writers may communicate reflections on life which are so banal and commonplace that they would arouse a derisive titter in a Sunday School class; men of the world may make gross errors of fact about the times they lived in, as when Bacon told Mr. Allen that in his Elizabethan incarnation he was a friend of William Wykeham, Bishop of Winchester (who had been dead 150 years when Elizabeth I came to the throne) (1).

There is indeed plenty of ground for being somewhat on one's guard against too easily accepting the identity of communicators. If any of us started receiving such messages on our telephones, we also would be doubtful about who was on the other end of the line. We should want to develop reliable methods of being sure as to whom we were talking to, not only so that we might not be deceived by bogus messages but also, and more importantly, so that when we really were talking to the right person we should not mistake his messages for bogus ones.

This problem of the evidence for survival is one that engaged the attention of our predecessors very intensely at one stage of the history of our Society, and the system of cross-correspondences reported in our *Proceedings* from 1906 onwards remains an important step on the way to obtaining proof of survival. I shall not say more about this work because I do not know a great deal about it. Its length and the difficulty of assessment (particularly for one who is not a Classical scholar) have prevented me from having made a close study of it. I understand, however, that, in the opinion of competent and critical judges, it is considered to point strongly to the unifying activity of a surviving mind rich in Classical knowledge which may well have been that of one or more of the ostensible communicators: Myers, Butcher, and Verrall.

I do, however, know a good deal more about Whately Carington's attempt at experimental testing of the autonomy of communicators by finding out whether they gave a pattern of response to a word-association test characteristically different from that of the medium (**5**). He was inclined to believe when he published these results that he had found the required evidence, but unfortunately critical examination of his figures showed that there was no more difference between his results for different communicators than would be expected on the hypothesis that there was no difference between his communicators (**11**).

If Mr. Whately Carington's experiments had succeeded, and if their success had proved what he set out to prove, what would have been established would, of course, have been merely the autonomy of his communicators and not their identity. The result would have been quite consistent with the hypothesis that the communicators were, for example, discarnate spirits but not the surviving spirits of the ostensible communicators. He makes it clear, however, in his *Chambers' Encyclopaedia* article on "Survival of Death," that he projected a further experiment along the same lines to establish the identity of communicators by applying the same test to living people and, if one of them died and purported to communicate through a medium, to repeat the test with the intention of establishing whether there was significant resemblance between the living and the post-mortem responses (**6**). This experiment was, so far as I know, never carried out, but it remains a project which we hope may be taken up by a future research worker.

The problem of identification of communicators by correctness of information supplied is not so easy as it appears at first, even if we ignore the possibility of correct information coming by the psi capacities of the medium, and it is not until recently that we have had satisfactory methods of evaluating the likelihood of chance coincidence. In an ingeniously devised experiment, Mr. Parsons has shown that information which seems to sitters to be convincingly appropriate to their own case may also be accepted by other people for whom they were not intended (**10**). This fact obviously throws doubt on the evidential value of sitters' acceptance of information as

appropriate to their own case. This is another form of the problem familiar to us in card-guessing experiments, the danger of being misled by chance coincidences. The general principle of the method of dealing with this difficulty is, of course, familiar enough. It is that of devising a statistical method of treating the data to show whether the degree of coincidence is greater than can reasonably be attributed to chance.

In this particular case, the statistical problem proved to be a somewhat intractable one. The first step towards its solution was made by Saltmarsh in 1930 (**13**). However, neither this method nor a later improvement of it by Saltmarsh and Soal (**14**) was free from objections, and a satisfactory method for assessing mediumistic material statistically was not arrived at until 1948 when Pratt and Birge published a method based on Greville's method of evaluating multiple matches in card-guessing material (**11**). There has, so far as I know, been no new research based on this method during the short time that has passed since its publication. It is satisfactory, however, to have the method ready for any research worker willing to take up again this problem. It does not, of course, profess to give an answer to the final question of the identity of the communicator, but only to answer the preliminary question of whether there is demonstrable evidence of any paranormal process entering at all into the information given at a séance.

A more direct attack on the problem of proving the identity of the communicator was made by those members of the Society who left sealed packages with the intention of communicating their contents through mediums after their death. There have been several such experiments, and I think I am right in saying that none has succeeded. The latest is that of Sir Oliver Lodge who left a system of sealed packages, the inner one of which contained a description of a trivial incident which he expected to be able to remember and which could be known by no one except himself. Although there has now been a series of sittings with mediums in which Sir Oliver Lodge has appeared as ostensible communicator, he has given no clear indication of what is in the inner package.

Our experience with the Lodge package makes clear, I think, that there is a fundamental weakness in this form of experiment. One

can only open the package once, and when it is opened the experiment is finished since the contents are known. One needs a form of experiment in which repeated attempts at verification might be made without spoiling the experiment. There is a further difficulty that if the experiment is widely known, one gets many and diverse responses from various sources, and it is unlikely that some will not by chance bear some resemblance to the contents of the package. It follows that when the package is opened, unless there has also been a clear and unambiguously correct statement of the contents, the openers will be faced with the insoluble problem of assessing how far any resemblances between some of the statements and the actual contents of the package may be due to chance.

Experience of the difficulties of the Lodge package convinced me that if experiments of this sort are to be tried in future, two conditions must be fulfilled. First, what is to be communicated must be something which is plainly a hit or a miss with no partial resemblances. Secondly, there must be the possibility of repeated attempts at verification of possibly erroneous messages without spoiling the experiment. These conditions seemed best fulfilled by an experiment in which what was left was an enciphered message not enclosed in a package but open to everybody, and what was to be communicated was a key to the cipher (**17, 15**). It is quite possible to have a system of encipherment which is virtually unbreakable by rational inference if (and only if) one message only is enciphered by its means. Two such systems are described in my article. The first message (p. 253 of my first article) was enciphered by a defective system which was not unbreakable. So two tests remain; that on p. 258 of my first article and the one contained in my second article. The idea is that after my death I shall attempt to communicate in each case the key which will enable the message to be read.

All experiments of this type are, of course, open to the objection that success might be due to the psi capacities of the medium and not to the survival of the person leaving the message. I think this objection is largely overcome by the consideration that my passages are now available to decipherment by the psi capacities of mediums, and will remain so for the rest of my life. If they are not read during

my lifetime but the correct key is obtained after my death when the only change in the situation will be that my body will be dead and that I, if surviving, shall be desirous of transmitting the message, then I think it will give strong reason for supposing that it is my surviving spirit that is giving the message.

When I describe this experiment my hearers very commonly make the kind observation that they hope it will be a very long time before the final stage of the experiment takes place. So do I, not only because I have the usual natural wish to go on living but also because it is an essential condition of the experiment that there should be sufficient time between the printing of the passages and the obtaining of the key to make it reasonably certain that while I am in this body and unwilling to communicate the key, it does not come by paranormal cognition from mediums or other types of psychically gifted individuals.

It has been suggested by Mrs. Goldney that this part of the experiment would be best carried out if members of the Society would coöperate by asking controls at séances whether they can find any communicator who will give the key. Special séances are not necessary, but members who are attending séances for other purposes can ask for the key to my ciphers and send to the Society for Psychical Research a note that the attempt has been made and any response that has been obtained. It is desirable that all attempts should be recorded whether the results are positive or negative. It is obviously desirable that the attempt should be made under as nearly as possible the same conditions as would be the final attempts after my death. I suggest that the passages should be referred to as Passage II and Passage III (Passage I having been now discarded) and that the communicator should be told the nature of the key expected. For passage II this is the title of some literary work and for passage III it is two words. The passages themselves and the method of encipherment need not be communicated. For subsequent verification, these will be found in the two articles to which I have already referred.

It is, of course, obvious that failure to communicate may have other explanations than non-survival. It might well be that there is

considerable difficulty in remembering after death. That death entails the loss of the physical brain and that we do in this life depend very largely on our physical brains for remembering is an obvious factor that may limit the post-mortem remembering of departed spirits. There may be other difficulties in communicating of which we know nothing. Any experiment we can plan is devised on the simple hypothesis that any life beyond the grave can be regarded as a continuance in time of a separate personality such as we know in this life. This may not be the character of eternal life. Failure to get empirical evidence of survival might be due to the hypothesis tested being based on a theoretical model too simple to be adequate to the reality.

I think, however, that we should continue to try such experiments until either we obtain success or we have tried unsuccessfully so often that we may reasonably suppose that success along these lines is not to be obtained. I welcome, therefore, the coöperation of Mr. T. E. Wood who has also printed a cipher message in our *Proceedings* (**20**), the key to which he will try to communicate after his death.

It is perhaps hardly necessary to say, after Mr. Tyrrell's illuminating discussion of apparitions in his Myers Memorial Lecture of 1942 (**19**), that the reports of apparitions of the departed, even when well attested, do not in themselves provide evidence for survival of any weight. Whatever may be the explanation of such apparitions, there seems to be no reason for supposing that they are visible appearances of the spirits of the dead. It seems unlikely that the presence of a spirit without material or quasi-material substance would be visible at all. Moreover, the automatic and repetitive character of the reported behavior and speech of apparitions, to which Mr. Tyrrell has drawn attention, seems most easily explained if we suppose that these appearances are not directed by any kind of intelligent will.

I think that this question of empirical evidence for survival is given the wrong kind of valuation if its importance is supposed to be that its solution would give "a scientific foundation to religion." Obviously any true factual statement in a religious system of belief cannot be in conflict with any true factual statement in the system of the sciences. If the statement of the survival of the human spirit is

an essential element in religious belief, the religious believer must expect that it would be confirmed as a factual statement in a complete scientific system. But neither his religious belief nor his religious practices have to wait for that confirmation. If the goal of religion is (as we are authoritatively told) that man should love God and his neighbor, the mere empirical demonstration of the fact of survival will not in itself bring men nearer to that goal, nor has it happened in the past that men have been prevented from traveling along the road to that goal by absence of scientific confirmation of the survival hypothesis. I suggest that the proper reason for trying to find scientific evidence on the problem of survival lies in its importance for science not for religion. If it is a fact that the departed survive and can communicate with us, we want to have proof of that fact because without such proof our science is incomplete.

Another subsidiary object in the minds of our founders was to make psychical research a respectable subject of academic inquiry. The early investigators of psychical phenomena (as, for example, Crookes) imperilled their academic reputations when they let it be known that they were actively interested in psychical research, and they were subject to attacks on their competency and honesty which seem incredible now. The Society for Psychical Research was headed from the beginning by men of academic standing who could not easily be supposed to be pursuing a ridiculous or trivial inquiry. Throughout its history, the Society has not lacked the authority of men of intellectual distinction—Sidgwick, Lodge, Rayleigh, Myers, to name only a few—and this authority has had its influence on outside opinion. One still sometimes hears the suggestion that the academic reputation of any young scientist will be tarnished if he is known to be associated with psychical research. Perhaps it will be a little dimmed, but much less so than it would have been seventy years ago. For this greater freedom we have reason to be grateful for the courage and self-sacrifice of the leaders of psychical research in the past.

I suppose that in 1882 it would have been unbelievable that a university should have awarded a doctorate for an investigation in psychical research. There have now been many such from a number of universities including Oxford, Cambridge, and London. A doctorate

thesis on psychical research may, of course, be turned down because it does not come up to the exacting standards required for a doctorate; it will not in general be refused consideration because of its subject matter.

If one still finds prejudice against the subject itself in some academic circles, I think it is fair to say that it is generally least amongst those whose opinion we should be inclined on other grounds to value most highly. The grave interest of Professor X., who has made distinguished contributions to knowledge in his own subject, may compensate the psychical researcher for the derisive laughter of Dr. Gigadibs who perhaps owes his academic position largely to the correctness with which he keeps his feet on approved academic pathways.

Nevertheless, if a research student tells me that he wants to do an investigation in psychical research, I still think it my duty to advise him against it in his own interests, because it will be at best a blind alley leading to no certain way of earning his living. It may even be an impediment to his academic advancement, since Dr. Gigadibs may be sitting on the appointments committee when he applies for his first post. I am inclined to tell him that the path of worldly wisdom is to establish himself in some other field of study and then to do parapsychological experiments in his spare time.

If he chooses to ignore this advice and says that psychical research seems to him to be the one thing in the world that matters, and if also he has the technical skills required for this kind of research, then one has to let him go his own way. One may indeed rejoice that he is driven by the sort of demon that impels men to worthwhile research although it may not conduct them to worldly prosperity.

Let us now return to our original question. How far have the problems with which our founders started been solved, and are we on the right road for solving those still unsolved? Looking back on seventy years of psychical research, I think we have grounds for some satisfaction but not for complacency. A good deal has been done and we do not now stand in the same position as our founders occupied in 1882. But much remains to be done, and I think perhaps that in many directions less has been accomplished in seventy years than our founders would have hoped. This may well be due to the diffi-

culty of the subject rather than to misdirection of effort. It is to be hoped that when we celebrate our centenary in thirty years time, the Myers Memorial Lecturer of that date will be able to report a record of solid achievement during those years, with definite answers to some of the questions that are still unanswered today. It is safe also to predict that he will report new questions raised during those years which will give psychical researchers sufficient to occupy themselves with during the thirty years to follow.

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G. N. M. TYRRELL AND HIS CONTRIBUTIONS TO PSYCHICAL RESEARCH[1]

By W. H. Salter, G. W. Fisk, and H. H. Price

George Nugent Merle Tyrrell, who died on October 29, 1952, was born in 1879 and was educated at Haileybury, Seafield Engineering College, and London University, where he took a degree in Physics and Mathematics. He was one of the early students of wireless under Marconi, and when he joined the Society in 1908 he was in Mexico, demonstrating the working of the Marconi system to the Government. He served in the First World War as signals officer in the Royal Artillery, and was twice mentioned in dispatches.

After the war psychical research grew to be the main interest of his life. In 1921 he conducted a short series of experiments with Miss Gertrude Johnson. She had been known to him and Mrs. Tyrrell for several years and later became a member of their family. Tyrrell often discussed his projected books with her, and greatly valued her advice. In the report of the experiments printed in the S.P.R. *Journal* for June, 1922, Tyrrell is called "Mr. T." and Miss Johnson "Miss Nancy Sinclair." It is doubtless because the real names were not given at the time that in the great interest aroused by Tyrrell's later experiments with Miss Johnson this earlier work has often been overlooked.

The report throws much light on the way Tyrrell's interest in psychical research developed. His was not the sort of mind that would in any circumstances have contented itself with a narrow view of its objects and methods, but any tendency in that direction, had it ever existed, would have been checked by the variety of Miss Johnson's phenomena. She wrote automatically in trances of varying intensity; she had spontaneous flashes of apparent telepathy and precognition of the kind that create an impression no rational observer would disregard, even though they could not be quoted as evidence; and she saw visions in a crystal.

[1] Reprinted from the *Journal of the Society for Psychical Research*, March-April, 1953, by permission of the authors and the Council of the Society. The authors, all of whom are members of the Council, have long been associated with Mr. Tyrrell and have themselves for a considerable time been actively engaged in parapsychology.—Ed.

The most interesting part of the report, however, relates to experiments with playing cards. In some of the experiments Miss Johnson read off the values of the top cards of an already shuffled pack; in others she would specify a series of cards, and on a later day, the cards having been shuffled by Mr. or Mrs. Tyrrell, her forecast would be checked. With either method her scores enormously exceeded chance probability.

In experiments within the family circle there is often some informality which invites criticism that the conditions may not have been up to laboratory standards. In his later experiments Tyrrell was at pains to forestall objections of this sort by efforts to perfect the methods used, and particularly by devising apparatus of various kinds, referred to below, but the significant point of the 1921 experiments is that they were quantitative. Several of the experiments conducted in the early years of the Society were of that nature, but for many years before 1921 the attention of psychical researchers had been almost entirely turned in other directions. In reviving, therefore, interest in quantitative experiment Tyrrell may be regarded as the pioneer of a form of research which has since become of primary importance both in this country and in the United States.

For several years, however, circumstances did not permit the continuance of the experiments with Miss Johnson. By the time Tyrrell resumed them in 1934, quantitative research was well established. In 1928 Miss Jephson's important paper on Clairvoyance had been published (S.P.R. *Proceedings,* XXXVIII) and in 1934 appeared Professor Rhine's *Extrasensory Perception.* The main problems now facing Tyrrell were these: to devise an experimental technique adapted to Miss Johnson's powers in such a way as to elicit the maximum of exact knowledge of them; to ascertain, as the Council invited him to do in 1935, whether the results obtained with her could be repeated with other subjects; and to explore methods of differentiating between different types of ESP. With these objects in view he devised first the pointer apparatus and then the electrical apparatus mentioned below by Mr. G. W. Fisk, who was closely associated with his experiments.

In his Presidential Address, delivered in June, 1945, Tyrrell spoke of the danger of psychical research splitting into two groups

of statistical and qualitative workers, and added: "It is unnecessary that there should be this division, but it is likely to occur because two different types of mind and two different temperaments are involved . . . we need both types of mind in psychical research. Unfortunately, they are seldom united in the same individual." Fortunately, they were united, so far as that is possible, in Tyrrell. He gave abundant proof of this when, having been appointed Myers Memorial Lecturer for 1942, he took the opportunity of making a much-needed review of the subject of apparitions. These had provided the material for the first inquiry on a large scale undertaken by the Society, and in 1886 the results were published in *Phantasms of the Living.* In 1923 Mrs. Sidgwick made an important collection and analysis of cases printed by the Society since 1886, but a review of the theoretical implications of this kind of phenomenon had long been overdue. On this subject Tyrrell's lecture *Apparitions* is likely to remain a classic for many years.

Professor Price is contributing a discussion of the theoretical and philosophical aspects of Tyrrell's work, and I gladly leave those topics to him, but any tributes to Tyrrell would be incomplete without mention of his outstanding ability as an exponent of psychical research to the general reader. The letters received by the Society tell a melancholy story of intelligent inquirers making their first acquaintance with psychical research through literature crammed with unverified and unverifiable "facts" loosely strung together to support a sensational conclusion. It is useless to invite the inquirer to garner the truth for himself from the 49 volumes of our *Proceedings* and the 36 volumes of our *Journal.* It is accordingly an immense relief to be able to refer him to comprehensive surveys of the evidence as well arranged and as acutely discussed as Tyrrell's *Science and Psychical Phenomena* (1938) and the Pelican edition of *The Personality of Man* (1946). Nothing as good in their line as these books had appeared for many years, not, I would say, since the publication of Myers's *Human Personality* (1903). In style they well reflect the cast of Tyrrell's mind, modest, thoughtful, quietly humorous.

W. H. SALTER

Experimental Work

I became acquainted with Mr. Tyrrell and his experimental work in 1935. He had invited me to act as experimenter and agent with Miss Johnson to see if she would achieve significant results with agents other than himself. I was very glad to do so. Trials were made with ESP cards and also with the "Pointer apparatus." Both methods yielded significant results, although the deviations from chance expectation were somewhat smaller than those obtained when Mr. Tyrrell was in control.

Perhaps I should explain for the benefit of those who are unacquainted with Mr. Tyrrell's unique contributions to ESP experimental research that he initiated the technique of a subject "finding" an object, under controlled conditions, in lieu of "guessing" a design or pattern with ESP cards or "identifying" drawings, etc. He had noticed that Miss Johnson had a flair for finding lost objects in the home and designed apparatus to test that natural aptitude. The Pointer apparatus was an early model. It was quite simple, and consisted of five small boxes with hinged lids mounted behind a board in such a way that a pointer could be thrust through holes in the board into any selected box without the subject being able to see what the operator was doing. The subject was required to find, by raising the lid, the box into which the pointer had been inserted, the probability of success being, of course, one in five.

Two main lines of criticism were directed against the Pointer apparatus technique. There was the possibility of sensory clues being given to the subject—such as some sound being made by the pointer entering a box no matter how carefully it was done and how thoroughly the surfaces were padded. There was also the possibility of the operator's and subject's number habits coinciding and so producing a fictitious score. But Mr. Tyrrell was his own chief critic. He was always weighing in his mind every possible source of error; ever ready to listen to any criticism or suggestion, balancing the pros and cons with his own fine judicial mind. He would even listen patiently to the half-baked criticisms of a tyro like myself and then would interrupt my halting exposition with a "Don't you mean this?" and proceed to elaborate my point with that lucidity of thought and utterance that were so habitual with him. I recall how on one occa-

sion when I was able to demonstrate a method of wangling a false score with his later electric machine, he became as excited as I was and congratulated me with enthusiasm. He then immediately set to work to think the matter out, and eventually to modify his technique so as to make such a wangle impossible. That gave me an insight into his attitude as an experimenter which I have never forgotten.

The original Pointer apparatus was soon greatly elaborated and improved. The five boxes were retained, but each box was fitted with a tiny electric lamp which glowed when a corresponding key was pressed by the operator. The number of trials, successes, and failures were recorded automatically on a paper tape. The electric wiring allowed the separation of operator and subject by any required distance. From time to time, as suggestions were made, other refinements were introduced. No one could have been more eager than Mr. Tyrrell to adapt the apparatus to meet even remotely plausible criticism. Thus the final machine had, among many minor improvements, (*a*) a special switch that jumbled the wires from the keys so that the operator could not know which lamp was lighting when any one of the five keys was pressed, (*b*) a delayed action relay so arranged that no lamp would light up until *after* the subject had actually raised the lid of the correct box although the circuit determining which lamp was involved had already been fixed by the apparatus, (*c*) a mechanical selector which determined the operation of the keys in a random order.

Miss Johnson continued to score significantly above chance expectation no matter what alterations were made in the apparatus, although with the introduction of any change the score generally fell off for a longer or shorter period, until the psi faculty, as it were, became used to the new conditions.

So Mr. Tyrrell had thus devised and cut a new and intricate key with which to test the locks of the closed doors of knowledge. One could reasonably hope that new light would continue to be shone on the conditions that determine the emergence of psi phenomena. Then the war broke out and one of Hitler's bombs smashed everything. But it was not altogether a misfortune, for it meant that the experimenter would retire and yield undisputed place to the author and philosopher. That bomb gave Mr. Tyrrell time to write his

paper on apparitions and the books which are now known and studied all over the world.

G. W. Fisk

Theoretical and Philosophical Aspects

Tyrrell conceived of psychical research as "the exploration of human personality," and he was deeply impressed by the *queerness* of the conclusions to which this exploration seems to commit us. He thought they were queer not only in detail (in the way that the duck-billed platypus is a queer animal) but in principle, because they just would not fit at all into our current world-outlook. In order to make sense of them—indeed, in order to get the facts themselves accepted—a kind of intellectual revolution would be needed. This is a recurrent theme in all his writings.

Is there any prospect of such a revolution? On this point we can trace a certain ambivalence of view in Tyrrell's work, an oscillation between hope and despair. The pessimistic view, most clearly stated in *Homo Faber,* is a kind of Kantian one, though it is also influenced by Tyrrell's study of Bergson and Bradley. In this mood, he maintains that the queer facts discovered by psychical research merely reinforce the conclusions to which reflection on normal experience ought in any case to have brought us, namely that our ordinary beliefs about the world and about ourselves are based on biologically useful illusions. The function of the human intellect, as of the human senses, is merely to adjust *homo faber* to his environment. The chief illusion is the biologically instilled assumption that the perceptible world is absolutely real and independent of ourselves, whereas in fact it is only a phenomenal world, distorted and artificially simplified by the character of our perceptual and intellectual faculties. Not only the sensible qualities we seem to find in it, but also the principles of order it seems to display—space, time, matter, causality—are imposed upon it by "the adapted mind." Of course there *is* a Reality—a real universe and a real self—lying behind the phenomenal world. Tyrrell never doubts this, any more than Kant did. But what that Reality is, the human mind can never grasp. In particular, human personality, as it *really* is, is something

"incomprehensible," "beyond our understanding." We simply have not got the concepts which would be needed for making it comprehensible, and we have no means of acquiring them.

It is true that even when Tyrrell takes his pessimistic line, he does always admit that there is a mode of awareness, in all men or at least in some, which transcends the limitations and the illusions of the "adapted mind." In mystics, in men of genius, in poets, and even occasionally in the ordinary man, there are intimations of something other than and deeper than the phenomenal world. But in his pessimistic mood, Tyrrell seems to think that this awareness, which raises us occasionally above the biologically conditioned myopia of *homo faber,* is something purely intuitive and incapable of being conceptualized; something which can express itself at the best only in symbols and in metaphors, and is therefore useless for the purpose of constructing a theory, or an explanation of anything approaching a theoretical understanding of human personality. Consequently there could never be a *science* of psychical research; or if there could, the most it could do would be to establish that supernormal events occur, and it could never do anything to make them intelligible.

This theory has its difficulties. The most important one arises from the conception of "adapted mind" itself. All the evidence we have for the proposition that living creatures are adapted, mentally or physically, to their environment is evidence which comes from within the world which sense-perception discloses. But if this world is merely phenomenal, not absolutely real, adaptation itself is merely a phenomenal relation between phenomenal entities. In the world as it really is, there are neither organisms nor an environment, though in the world as sense-perception discloses it there appear to be both. We note too that according to Tyrrell the "adapted" character of the human mind is caused by an evolutionary process. But according to his own theory the concept of cause only has application *within* the phenomenal world. Evolution too must surely be a temporal process. But according to the theory, the concept of time applies only to phenomenal entities; the real world is non-temporal or super-temporal. It would seem that Tyrrell is using Realistic premises to establish an Idealistic conclusion. In his premises, he asserts that the human mind *really is* adapted to an environment

which it *really has*, and that this adaptation *really was* the result of an evolutionary process. But his conclusion commits him to the view that the evolutionary process, the environment, and even living organisms themselves are merely phenomenal, appearances (and misleading appearances) of a Reality whose intrinsic nature is unknown to us. To put it in Kantian language: in his premises he is claiming a knowledge of "Things in Themselves" (a *trans*-phenomenal knowledge), which he cannot possibly have if his own Idealistic conclusion is true. And this knowledge cannot be just an intuitive awareness, incapable of conceptual formulation; it must be sufficiently conceptualized to serve as the basis of an elaborate philosophical argument.

But this quasi-Kantian theory, with the pessimistic conclusions about the prospects of psychical research which follow from it, is only one strand in Tyrrell's thought; and even if he had stuck to it wholeheartedly, his own constructive work as a psychical researcher would be the best refutation of such an agnostic philosophy. *Solvitur ambulando.* He shows that "the exploration of human personality" is a possible enterprise for the human intellect by actually doing it. He shows us that illuminating explanatory concepts can be produced by actually producing them. He shows us that the mysterious depths of human personality are *not* altogether "beyond our understanding" by actually helping us to understand them a good deal better than we did before. First, he points out that human personality, when seen in the light which supernormal phenomena throw upon it, turns out to be something vastly more complex than we had supposed. We should conceive of it as a "many-levelled" or "many-graded" hierarchy. As he sometimes says, there are "degrees of I-ness" in it. For example, a man's instinctive desires are "more I" than his body, and his body is "more I" than his clothes. What is "most I"—the Pure Ego of the philosophers, the Atman of the Hindu mystics—may indeed remain incomprehensible to the end. But we can hope for some comprehension of the intermediate levels which lie between the Pure Ego at the one end and the material organism at the other—provided we try to form ideas which will fit the facts instead of forcing the facts to fit our preconceived ideas.

Those considerations led Tyrrell to suggest that supernormal cognition occurs in two distinct stages. The telepathic contact between

two personalities is something which occurs, or exists, at the unconscious level; and then, in addition, the results of it have to be "signalled" to consciousness. Tyrrell has interesting things to say about both stages. The first, though one may use words like "contact" and "reception" to describe it, is not to be thought of as a relation between two personalities which were initially separate, and the spatial prefix *tele-* is misleading. Nothing "passes" or "is transferred" from the one mind to the other, because at the deeper levels of the personality there is no separation between mind and mind at all. In the second stage, the crucial process is one of imaginative dramatization, which may take many different forms, and sometimes highly symbolic ones. Moreover, what is telepathically received is not a cut-and-dried "message," but a relatively general idea or "theme"; and this has to be worked out in detail (and perhaps distorted in the process) by the unconscious imaginative powers of the recipient, humorously personified under the names of "the producer" and "the stage-carpenter" in the lecture on *Apparitions*. This process of detailed imaginative dramatization is seen at its best in the production of full-blown telepathic apparitions, complete with apparitional clothes and other apparitional appurtenances (e.g., horses, carriages) to suit them. But something like it, this time in a verbal form, occurs in mediumistic communications too. Tyrrell draws two important conclusions from this. On the one hand, no mediumistic communication can be taken at its face value. It is not at all like a communication by telephone. On the other hand, the two rival explanations of the facts, the Survivalist one, and the Naturalistic one in terms of secondary personalities, are not so sharply opposed as they look. Who or what is communicating? Is it a discarnate personality, or is it just a secondary personality of the medium? The answer may be that it is a temporary and *ad hoc* personality which is a blend of both; and the two constituents of it might be blended in different proportions in different cases. As he says at the end of the lecture on *Apparitions* (p. 122) his personal impression of the work which has been done on the problem of survival since 1882 is that the crude question whether we survive death "has been rubbed off the slate . . . and instead of a direct answer, we have had revealed to us something of the general perspective in which the question

ought to be asked." This impression seems a very just one. But it should be added that Tyrrell's own work has contributed very notably to this revelation of the appropriate "general perspective."

If we consider the constructive suggestions which Tyrrell himself has made, we can see why the deeper levels of human personality, and the supernormal occurrences which have their source there are liable to appear "incomprehensible." The fault does not lie with *homo faber,* but with Descartes. The "incomprehensibility" of the phenomena is not due to any inherent limitation of our intellectual powers, as Tyrrell, in his pessimistic mood, supposed it was. It is something historically conditioned, a result of the climate of opinion which has prevailed among educated Western Europeans from the middle of the seventeenth century to our own day. Descartes did more than anyone to establish this climate of opinion, and his theory of the human mind was a peculiarly narrow and rigid one. A human mind, he thought, is an indivisible psychical substance whose essential attribute is consciousness. From this it follows that the concept of unconscious mental activity is a self-contradictory one, and it is likewise self-contradictory to suppose that a human personality might split into two or more sub-personalities. Moreover, he thought that a human mind is causally insulated from all other finite minds; the only causal relations in which it could conceivably stand were relations with its own brain and with God. From this it follows that telepathy is a self-contradictory conception, and of course psychokinesis also. Though later philosophers protested against this Cartesian theory, it came to be accepted as part of Western European "common sense." It is these inherited Cartesian preconceptions which have made the phenomena of psychical research appear incomprehensible; and indeed the phenomena of psychopathology too, though the educated public is willing to put up with these, under protest, because it has turned out that the investigation of them has practical therapeutic value. We must get rid of these Cartesian preconceptions if we are ever to make any progress with the theoretical side of our subject. It is very difficult to get rid of them, and still more difficult to think of something more adequate to take their place. But it is not impossible, as Tyrrell's own constructive work has shown.

H. H. Price

THE HOMING PROBLEM IN PIGEONS

J. G. PRATT[1]

HOW DO BIRDS guide their flight when migrating over distances of thousands of miles? How are they able to return home when taken hundreds of miles into strange territory? These questions have puzzled observers for centuries. There has been no lack of speculation about the answers, but most of the suggestions have been sheer guesses, unsupported by the kind of careful analysis and observation that would give them the status of serious scientific hypotheses.

Recently, however, there have been growing indications that scientists have finally accepted the challenge presented by the unexplained facts of bird orientation. There is a rising wave of research interest in the problem. These investigations have continued long enough to show that the interest is a persistent one, and their present scope justifies the hope that scientists have at last claimed a problem that heretofore has been largely left to pigeon fanciers, nature lovers, and poets.

In this research, the most promising work has been in studies of homing, as opposed to the migratory flights of wild birds. Homing refers to the ability of certain species of animals to find their way back to the nest or familiar surroundings after they have been taken away and released in strange territory. No one questions the fact that many animals are able to home, though there are apparently wide differences among the species in regard to this ability. However, there have been widely divergent opinions on how homing performances are to be interpreted. One school of thought has held that the facts do not present a really deep scientific mystery. Another group has assumed that there is a real problem in homing but that the solution must surely lie in undiscovered sensory functions of the animals. The question becomes one for parapsychology only if all efforts to find an explanation in terms of recognized physical principles should

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appear to show that no single sense or combination of sensory abilities can explain the facts. From the parapsychological point of view, the problem is whether homing may in some instances involve the use of psi capacities comparable to the ESP ability which has been established in man.

It is not my purpose to present a general discussion of the homing problem, but rather to review it in the light of recent experiments with homing pigeons. Professor Thorpe of Cambridge University appraised the situation in 1949, before the experiments to be reviewed were performed, as follows: "It seems probable then that all pigeon results are explicable on the basis of good vision, quick learning of landmarks and rapid and efficient methods of good exploration" (**13**). This statement, even though only tentative, reflects a high degree of confidence that the outlines of the explanation of pigeon homing were already apparent. The solution of the problem, in other words, did not then seem to him to depend upon the discovery of any new basis of orientation, but only required further study of how the pigeons used recognized abilities in finding their way. Since that time, there have been some surprising developments regarding the problem of pigeon homing as a result of the work of Matthews (**10**) at Cambridge University; of Kramer and von St. Paul (**9**) and, again, of Kramer (**8**) at the Max-Planck-Institut in Wilhelmshaven; and of Hitchcock (**6**) at Middlebury College. The work of these investigators, instead of advancing our understanding of pigeon homing in terms of the principles mentioned by Thorpe, has shown that this behavior will have to be explained in quite other terms.[1] And it is far from apparent at the present time what the explanation may be.

In the present paper I propose to review the bearing of the recent experimental findings upon hypotheses of pigeon homing. A part of this objective will be to ask whether, in the light of the present facts, the explanation may lie in some revision or combination of the recognized hypotheses or in some sensory hypothesis that has not yet been

[1] The extent to which the recent work has shifted the focus of the homing problem is illustrated by two review articles by Griffin. In an extensive survey of the evidence in 1944 (**4**) he took essentially the same position as that of Prof. Thorpe, stated above; but in another review article in 1952 (**5**) he recognizes that homing orientation at a distance in untrained animals is a fact, but one that has not yet been explained.

considered. Finally, I want to discuss the extent to which the present situation justifies consideration of the ESP hypothesis as possibly applying to pigeon homing.

General Results of Recent Pigeon Homing Studies

The basic experimental plan followed in the recent pigeon homing studies is quite simple. It springs from a recognition of the fact that homing represents a complex act consisting of different phases or aspects and that each of these may present quite distinct problems of behavior. Professor Thorpe identified these phases as: (1) the initial choice of the home direction; (2) holding to this course throughout the journey; (3) recognition of home territory and of the loft at the end of the journey. Like Professor Thorpe, I will comment on these in reverse order, as the first one is the one upon which the real crux of the homing problem is centered.

The third or final stage of homing, the recognition of home territory and the loft itself, does not seem to present any special problem. These features would be familiar from numerous exercise flights, and the facts as they now appear suggest the normal use of vision.

In regard to the second phase, recent studies of bird and insect orientation (**3, 14, 15**) indicate that many organisms can maintain a definite line of orientation with reference to the position of the sun in the sky, or even by means of a pattern of polarization of light in the blue sky. A simple "sun angle" hypothesis seems adequate to explain how a pigeon can fly on a given course once it has chosen a direction of flight. We do not know, of course, that this is the explanation of the way in which the pigeon guides its flight, but the plausibility of the sun angle hypothesis makes the problem of this second phase of homing behavior seem relatively less urgent than that of how the choice of the homeward direction is made in the first place.

A great advance in clarification of the problem was achieved in the recent experimental studies of pigeon homing with the recognition of the fact that the crux of the matter was this initial choice of

the homeward direction. A corresponding advance in methodology was the discovery that the initial flight orientation could be studied effectively by observing the bird with field glasses from the point of release and recording its behavior until it vanished from sight a mile or two away. Critical data on the first phase of homing could thus be recorded within a few seconds or minutes following the release.

The Matthews Experiments

Matthews took the lead in directing experimental attention to the first phase of pigeon homing, and he was the first to show by means of observation at the release point that pigeons can turn toward home in strange territory from any direction.

He based his experimental procedure upon the training methods traditionally used by pigeon racers. He worked with young birds of three to five months of age. They were first thoroughly settled in a loft and allowed to become familiar with the surroundings through numerous exercise flights in the immediate neighborhood of home. These birds were then given homing experience at increasing distances in one direction only, from the north. Releases along this line were continued until the birds had homed twice from a distance of 100 to 125 miles.

Critical tests of homing ability were then introduced without any interruption in the schedule of releases. The pigeons were taken in light-proof boxes and inside a closed van truck (as they had also been transported in the tests along the training line) 80 miles west of the loft. There the birds were released one at a time and observed until they were out of sight. They showed an unmistakable tendency to fly toward the east in the direction of home instead of south, as they had been trained. In the next test the birds were released to the south, a distance of 90 miles. Again most of them flew toward home, opposite to the direction of training.

Matthews conducted tests like these in 1949 and repeated them in 1950 with the same results: after preliminary training along one line his pigeons were able to turn toward home in a new direction.

The Experiments of Kramer and von St. Paul (1951)

Dr. Kramer and his collaborators, like Dr. Matthews, recognized that the basic problem of pigeon homing is centered in the choice of

the home direction, and they also relied chiefly upon observations from the release point. In one important respect, however, their work represented a distinct advance. Kramer and von St. Paul tested the homing ability of pigeons that had not previously received any direction training. They took birds with a minimum of homing experience (four to seven releases in different directions within 10 miles of the loft) and gave them test releases at a distance of 200 miles. Kramer himself expected only to find evidence of homing ability under these conditions from statistical tests applied to a vague homeward tendency, chiefly to be recognized from reports of where the birds were found. Nevertheless, he and von St. Paul in 1951 carried out an experiment along these lines. Special steps were taken during the three weeks preceding the test release to increase the homing motivation.

The pigeons of Kramer and von St. Paul, like those of Matthews, showed an unmistakable preference for the home direction at the release point. (Whereas Matthews had made single releases, Kramer and von St. Paul released groups of four to eight birds.) There was one striking difference in the departure flights: Matthews' pigeons circled and flew an irregular course before they finally went out of sight. Kramer and von St. Paul found, however, that their birds took a straight line of flight which usually deviated only slightly from the home direction.) Releases under an overcast sky in both experiments formed an exception that will be discussed later.) The reason for this difference in departure flights is not yet clear, but it may be relevant that the investigators in Germany used older, mated pigeons (most of them at least one year of age) and the flight groups contained birds of only one sex.

The Kramer and von St. Paul birds also showed up surprisingly well in reaching home. Of 27 birds with no previous experience with distance flights, 14 were back at the loft on the day of release, and others came in the following day. Only three out of these 27 birds with no previous releases beyond 10 miles were lost on the 200 mile flight.

Kramer's 1952 Experiment

This is mentioned in the present connection simply to take note of the fact that homing tests were made this year with the same basic procedure as in 1951 and with the same clearcut evidence of choice of home direction and excellence of homing performance. This experiment is chiefly important for its bearing on the sun navigation hypothesis and it will be described more fully when it is considered in this connection later in the review.

The Experiments of Hitchcock

Hitchcock followed flocks of pigeons by airplane. The pigeons were released in groups from the ground after the plane was in the air. The initial flights generally showed circling. The flight paths were charted as far as the birds could be kept in view. The birds showed a tendency to move in the general direction of the loft, but the flocks rarely took a direct line toward home. Nevertheless Hitchcock concluded, like Matthews and Kramer and von St. Paul, that pigeons show an appreciation of the homeward direction in strange territory.

On the Nature of Homing Ability

How is this homing ability to be explained? The recent experiments also produced important evidence pertaining to this question. This consists of results that bear upon different hypotheses of homing ability. These hypotheses and their status in the light of the new findings will now be considered.

The research on pigeon homing is at a point of transition. The experimental facts that are now available indicate beyond question that the term "homing" includes a class of behavior which presents a sharply challenging scientific problem. As yet, however, no one hypothesis has emerged as the explanation or even as the one most likely to be experimentally established. Under the circumstances, there is a real danger that the correct hypothesis might be overlooked while each investigator pursues his own preferred line of experimental attack. Until it is known what the solution *is*, there is reason for caution in saying what it is *not*. It is worth while, therefore, to

review all of the hypotheses that have received serious scientific consideration and to measure each one against the present facts.

The Hypothesis of Random Searching for Familiar Landmarks

This hypothesis implies that no true homing ability exists. As indicated previously, it was advanced with considerable confidence only a short time ago as an adequate explanation of all pigeon behavior. Since then it has been shown that pigeons may choose the homeward direction at the release point.

Clearly, this fact is directly opposed to the hypothesis of random searching. The only question that needs to be raised, therefore, is whether enough independent observations have been made to exclude accident as the basis of homing. Were the critical releases unquestionably made in strange territory for the pigeons, or beyond the range of vision of familiar landmarks? In Matthews' work, we may recall, the critical tests were made 80 miles west and 90 miles south of the loft, but only after the birds had been released repeatedly at increasing distances to the north. Could the birds have flown indirect courses from these earlier releases or have overflown the loft and thus have become familiar with the critical release points? Matthews excludes this possibility on a logical basis by showing that the birds which reached home more promptly on all previous releases were more accurate in turning toward home in the critical releases. The birds were consistently fast or slow homers throughout the tests; therefore, superior homing by some pigeons in the critical tests could not be interpreted as showing that those birds had taken enough time in the early stages of the experiment to explore the English countryside.

The results of Kramer and von St. Paul exclude in a still more definite way the use of familiar landmarks at the release points. Pigeons, a nonmigratory species, do not fly more than a few miles from home while exercising about the loft; at most they become familiar with territory within a radius of five miles and they may be supposed to be able to distinguish landmarks in relatively flat country not more than 25 miles. Therefore, the use of birds in test releases at a distance of 200 miles the first time they were transported beyond

10 miles clearly eliminates recognition of landmarks as a factor in orientation.

Under this general heading may be included the possibility that departure flights tend to be *directed* at the release point, but without the loft as an orienting factor. The observed flight behavior, in other words, might not be random for purely local reasons, and it is therefore necessary to ascertain whether any apparent relation between oriented departures and the home direction exceeds the limits of reasonable coincidence.

It is conceivable, for example, that if the birds flew high enough they might get an impression of the general topography of a large area. They might, for example, detect general features of the terrain which would indicate the direction of a coastal plain in which the loft was situated. It may be inferred from the behavior of Kramer's birds that they were not using this method. Most of the releases were made from the top of a castle tower in Gleiberg. This is in hilly country. In the 1952 experiments the birds were not permitted to see the landscape before they were released. Yet they showed definite flight orientation within 10 seconds after their release. During this brief time they were mostly flying downward from the top of the tower to pick up speed. The hypothesis of depending upon a far-ranging visual survey would require that they gain altitude before choosing a homeward direction. Homeward orientation was also clearly evident in releases made on the island of Fehmarn, 135 miles northeast of Wilhelmshaven. From that point any far-ranging survey would have revealed only the uniform, sea-level horizon. In this instance it is difficult to imagine that any distant topographical features might have revealed the general home direction.

Still another possibility is that strictly local topographical characteristics at the release point may tend to make the departure flights favor one direction. This may take place on some basis that is quite unrelated, psychologically, to the home direction. That is to say, the pigeons may be responding to local features in their own right, and not because they are mistaken for something similar back at the loft. Or a tendency for departure flights to favor one direction may be the result of the fact that something in the locality, such as a group

of buildings or a prominent grove, is mistaken for similar landmarks near the loft. Both Matthews and Kramer recognize this general possibility, and Kramer argues that it is disproved by his 1952 results. Among the releases made from Gleiberg at that time, only those that occurred while the sun was visible showed immediate flight orientation which favored the home direction. The others were made with an overcast sky that blotted out the sun but without limiting visibility. These flocks flew in circles and showed much less accurately directed departures. (Matthews reported a similar contrast in observations with clear and overcast skies.) Kramer argues that this difference in behavior occurred without any difference in local topography and therefore the definitely oriented flights when the sun was visible cannot be ascribed to unidentified geographical features. This line of reasoning seems to me to overlook the possibility that overcast skies may have an inhibiting effect upon the flight activity of pigeons. How would their initial flights compare under the two conditions, clear skies and heavy clouds, in territory with which the birds are clearly familiar? If any difference is found between these two conditions, it would suggest that terrain is not the only factor that affects flight even when it would be expected to be the predominating factor.

An adequate test of homing ability becomes more difficult if it is granted that pigeons may tend to depart in a given direction from any particular release point because of local topographical features which the human observer has not identified or which he may not even be able to recognize. The obvious answer to this point is to use a large number of distinct release points that have been selected on an appropriate basis. If each locality favors flight in one direction, the departures should be randomly distributed for a group of points. Therefore consistent choice of the loft direction regardless of the release point would indicate that home, and not the local terrain, was the basis of the behavior.

Has the number of distinct release points used heretofore been sufficient for this purpose? Kramer has used only two locations for releases, and Matthews indicates five places from which he made releases off the training line. (Hitchcock's report is not sufficiently clear regarding the initial flights and also regarding the previous

experience of the birds to bear decisively on this question.) Four of Matthews' critical release points were clustered within a radius of approximately 20 miles at a distance of 40 to 80 miles west of the loft. If each of the release locations used thus far is assumed to be geographically distinctive the recent investigations have provided only seven independent geographical observations. (An "observation" is the average effect of all the flights observed from a given place.) As far as can be judged from the reports (that of Matthews does not give the detailed results for each test release point), each location showed a clear favoring of the home direction in the departure flights. The probability that seven randomly selected release points would all have topographical features that would direct the initial flight of pigeons toward "home" (that is, not more than 90° to the left or right of the home line) is $(\frac{1}{2})^7$ or one time in 128 tests of this scope.

This result is sufficient to exclude local topography as the explanation if the release points were selected on a sufficiently random or objective basis. The information regarding how they were selected is not reported fully. Undoubtedly, the choices were made to facilitate observing the flights of the pigeons, but nothing is said about whether the positions had fewer obstructions in the homeward direction than in others, how the nearest city was situated, and the like. Choosing the release point on a subjective basis might tend to favor local topographical features that produce directed flights toward home. Until more is known about the factors influencing flights, it would be desirable to use a larger number of release points and to determine the order of using them on a random basis.

There is some justification for reserving judgment until the recent work has been confirmed. Unlikely as it seems, it is still conceivable that at the seven places used the departure flights were only "accidentally" pointed toward the loft. After the birds were beyond the range of local influences, they may have continued in the direction first taken. If this position appears to be too conservative by usual scientific standards, it may nevertheless be appropriate for findings as unexpected, and potentially of such revolutionary significance, as these pigeon homing results.

The Kinesthetic Hypothesis

This topic includes any explanation based on the fact that all homing tests start at the loft. The hypothesis states that orientation toward the loft is continuous throughout the enforced outward journey and until the animal regains home by its return flight. It is commonly assumed that the sense organ primarily involved in maintaining orientation is the system of semi-circular canals of the inner ear. To make this hypothesis seem more plausible, the suggestion has frequently been made that this set of organs could register not only the organism's changes of momentum but also the coriolis force resulting from the rotation of the earth, and that the latter might provide a basis for holding to a chosen line of flight. The possible role of coriolis force has been most recently discussed by Beecher (2), but the most thorough consideration of the theoretical possibilities for such a hypothesis has been given by Ising (7).

As long as homing tests involve first transporting the birds, there is no way in which the kinesthetic hypothesis can be eliminated in an absolute sense. (Perhaps there is the impracticable method of sending the bird on a great circle journey around the earth to be released at a distance of, say, 200 miles from home.) The hypothesis has been attacked experimentally, however, by subjecting some of the birds during the outward journey to conditions which would make the maintenance of orientation incomparably more difficult for them than for the other birds. One group, for example, would be transported under conditions that kept their cage in continuous rotation with changing speed during the outward journey. Their homing performance would then be compared with birds that were carried in the usual straightforward manner.

Matthews conducted experiments of this kind on some of his releases. He was not the first to use this principle, but he applied it to a larger number of birds than earlier investigators had used, and he was the first to pay particular attention to the departure directions. He found no difference in the ability of birds in the two groups to turn toward home.

Matthews' results, taken together with the similar ones of other investigators, appear to rule out the kinesthetic hypothesis. The only alternative seems to be one of claiming that the kinesthetic

impressions work in an absolute or infallible manner. Probably no one is prepared to go this far. This supposed basis of homing has been so weakened by the experimental facts that it is no longer of primary concern. It will again become important only if further research should reduce every other conceivable hypothesis to a level of equal or lower plausibility.

The Hypothesis of Homing by a "Magnetic" Sense

The idea that pigeons may home by means of some sensitivity to the earth's magnetic or electrical field was first advanced as a purely speculative hypothesis with little or no factual or logical justification. This suggestion achieved a scientific status when it was incorporated in a hypothesis by Yeagley and made the basis of an extensive investigation of homing (**16**). Yeagley's hypothesis assumed that the pigeon homes by means of sensitivity to two kinds of force detected during its flight: (1) the coriolis force already mentioned, and (2) an effect based on movement through the earth's magnetic field. Only the vertical component is thought to play a part. The strength of the magnetic field that might be involved can be calculated on a theoretical basis. As each of these hypothetical forces, coriolis and magnetic, was indispensable to the hypothesis, Yeagley conceived the idea of testing his theory by attaching maggets to the birds' wings as a control on this aspect. Magnets were used that would produce a magnetic field many times as strong as the bird might experience due to its flight alone. The logic of the experiment was that the strong and vacillating magnetic field generated experimentally by the motion of the wings would obscure the weak hypothetical effect which was supposed to play an essential role in homing. If birds with magnets failed to home while others carrying neutral weights were successful, the hypothesis would gain support; if both groups homed equally well, the evidence would be against it.

Yeagley's own experiments with magnets and weights were ambiguous, chiefly because they were conducted on such a small scale. Several investigators have since carried out similar tests and the results have shown that the magnets have no effect. Again, Matthews has done the most conclusive work along these lines. He has found

no difference of any importance whatsoever between his birds with magnets and those with control weights.

In the light of these tests the "magnetic" hypothesis of homing is in the same situation today as that stated for the kinesthetic hypothesis: the earth's magnetic field is not a likely factor in homing. Further tests of this hypothesis would appear to serve no useful purpose, at least not until other possible explanations of homing have been more thoroughly explored.

The Sun Navigation Hypothesis

Matthews, having in the course of his investigations of pigeon homing largely disproved the existing hypotheses, proceeded to propose a new explanation within a sensory framework. Kramer and his collaborators had already reported work showing that the position of the sun may be a factor in bird orientation in a particular compass direction. Matthews extended this principle another step and offered the hypothesis that birds might determine the location of any release point in relation to home from observations of the sun.

This hypothesis requires the assumption of many complex abilities in the pigeon. They must have a substitute for the human navigator's chronometer and navigational tables. They must be able to estimate sun angles within the general degree of accuracy achieved by means of the sextant. Finally, they must perform the equivalent of mathematical calculations in correlating the sun's data at the release point with those at the loft as known from, say, the preceding day.

The human navigator with his instruments and other aids (not including a compass) fixes his position by observing the sun at two different times, preferably separated by as much as 30 minutes or an hour. The pigeons' achievement as represented by this hypothesis would be ever so much more remarkable. The bird is supposed to see the relative change in the sun's position between the moment of release and the short time afterward—measured in seconds or a very few minutes—when the homeward orientation is evident. This interval of time in Matthews' experiment generally varied from two to ten minutes. During this interval the bird was on the wing in circling flight. The pigeon, therefore, had no set course, fixed point

of reference, or constant horizon (especially in hilly country) from which to estimate the essential characteristics of the sun's height and curve of apparent motion. The difficulties the pigeon would encounter in making such observations as this hypothesis requires would be greater at the low altitude at which it usually flies (often just above the treetops and seldom above 300 feet) than at higher levels. There is clearly a great difference between the abilities assumed to be operating in the pigeon under these circumstances and those used by the human navigator with the aid of his instruments, and it is only the absence of any other conceivable sensory explanation of homing that would justify serious scientific consideration of the sun navigation hypothesis.

Matthews' experiments produced one result that offered at least suggestive evidence against his hypothesis, though he does not discuss the finding in relation to this point. He observed that those birds that took the shortest time to disappear from sight (1½ to 3½ minutes) vanished on the average closer to the home direction. Thus his own data produced evidence that the quickest orientation was the best. This fact seems to be opposed to the sun navigation hypothesis for the following reason: it is essential, according to this hypothesis, that enough time should elapse for the bird to observe the relative motion of the sun, and the longer this time, the easier and more accurate the inferences regarding the home direction should be. We might expect, therefore, that accuracy of judgment would be directly related to the amount of time taken for making and checking observations of the sun, instead of the opposite, as Dr. Matthews found in his pigeons. Dr. Kramer has pointed out the alternative interpretation that the quickest departures may have been made by the birds that were most gifted in sun navigation. This consideration is an important one, and it keeps this evidence against sun navigation from being conclusive. Two comments on Dr. Kramer's qualification are: (1) Some minimum time interval after the sun is visible to the bird is necessary before the basic data for sun navigation would be available; (2) the best orientations were shown by birds that were lost from sight at about 1½ miles in 1½ to 3½ minutes, so they must have shown homeward orientation from the start of

their flights or within a few seconds after they were taken from the dark boxes.

Kramer and von St. Paul carried out their investigation of homing in untrained pigeons with a favorable attitude toward the sun navigation hypothesis. Their tests in 1951 were based upon only eight groups of pigeons. Four of these consisted of pigeons that had homed 80 miles from the south the preceding year, and four, of birds that had received only 10-mile, nondirectional flights about the loft. These were not enough groups for controlled tests of the sun navigation hypothesis. Accordingly, a plan was followed which allowed the birds full opportunity to make use of this hypothetical basis of homing. The birds were transported in open cages on the floor of a bus in such a way that they could see the sky throughout the journey, but not the landscape. At the release point the birds were placed on top of the tower in the same open cages and were left there throughout a full day, during which they could observe the sun.

Each group was released separately on the forenoon of the second day and every flight turned immediately toward home without any circling or hesitation. The cages were opened facing in different directions, and the course the birds took toward the north was so definite and so immediately evident as to suggest that the birds were already oriented before they left the cages. The main object of the experiment was to find out whether, under the most favorable conditions that could be provided, homing behavior could be obtained without direction training. The research was definitely successful in achieving this primary objective. The investigation was not planned to provide a crucial test of the sun navigation hypothesis; rather, it was assumed throughout that this might apply. The homing results were therefore consistent with this view but did not provide any direct evidence in support of it.

Kramer carried out further experiments in 1952 with a much larger number of flight groups and with a brilliantly designed procedure for testing whether the height of the sun is a factor in homeward orientation. A few of his groups were composed of pigeons that had been released at the same place (Gleiberg) in 1951, but most of the groups were made up of birds that had received only as many as five releases in different directions up to 10 miles about the loft.

Each group was released and allowed to fly out of sight while the others were under cover, and most of the flight groups were still intact and separate when they reached home. There was no possibility that the experienced birds set the example for the inexperienced one at the release point, and very little likelihood that the new ones were guided on any part of the homeward journey. All of the conclusions reached in his experiment may be based solely upon the results of the new birds.

The birds for these releases were transported in a closed truck with ventilation holes which admitted only indirect light. Inside lights were burning brightly enough to conceal the moments of sunrise and sunset.

The groups were separated into two classes that were subjected to different light conditions between the moment of reaching the destination and the time when the birds were actually released. The "light" groups were taken from the transport truck in the dead of night and were placed on top of the tower in open cages, as the 1951 birds had been. The "dark" groups were taken in shielded cages in the middle of the night into the tower. Each of these flight groups was kept inside the tower, carefully protected from either direct or indirect daylight, until 30 seconds before it was to be released. Special care was taken to prevent any outside light reaching the pigeons of the "dark" groups at the hours of sunrise or sunset, as the length of the day in the month of June might, according to the sun navigation hypothesis, provide a clue to the birds that they had been displaced toward the south.

At the appointed time for releasing one of the light groups, the cage was placed in position and the side wall of the cage was removed. (All other groups that were on the tower at the time of a release were prevented from seeing the departure flight.) As the moment for releasing a dark group approached, the cage was brought into position inside a light-proof screen by an assistant. On a given signal he moved it into the open and placed it on the platform of the tower for a period of 30 seconds to allow the pigeons to become accustomed to the light. Then the side wall of the cage was removed to release the birds in the same manner as with the light groups.

Both kinds of groups turned immediately toward Wilhelmshaven and went out of sight in the chosen direction. The courses deviated somewhat from the home direction, but by a surprisingly small angle in all except one or two instances, and even in those cases by less than 90°. There was no circling and very little deviation of the flights from a straight line course.

Some groups from each of the two classes as distinguished by light conditions were released at or quite near the time of the forenoon when the sun at Gleiberg was the same height above the horizon as it was at the same time at Wilhelmshaven. Other groups drawn from both light conditions were released at an earlier or later time of the forenoon, when the sun at Gleiberg was either lower or higher than an observer would have seen it at Wilhelmshaven. If the height of the sun was a factor in the orientation direction shown by the pigeons, they should have departed from Gleiberg at these three different times of day in directions showing clearly predictable errors of flight. No difference among the three release times was observed, and the conclusion from these results was that the height of the sun, within the limits involved in these releases, was not a factor in flight orientation. It should be noted that the differences in sun heights at Gleiberg and Wilhelmshaven at the earliest and latest release times were as great as one could ordinarily find for releases at a distance of 200 miles. Certainly if it was reasonable for Matthews to suppose that his birds might be responding to differences in the sun's position at 80 miles, Kramer's pigeons had a much better opportunity to make inferences regarding the home direction on this basis.

What may be said about the sun navigation hypothesis on the basis of these experimental results? Dr. Kramer says that he sees no opportunity of maintaining this hypothesis as formulated by Dr. Matthews unless it is assumed that the pigeon can carry out the complicated observations and calculations of the sun's position and relative movement in a period of 40 seconds. (This period covered the 30 seconds for light adaptation of the dark groups and 10 seconds after the release, by which time the homeward orientation of the birds in flight was already clearly apparent.) Furthermore, to save the sun hypothesis, it is necessary to suppose that the complicated judgment can be made just as well in 40 seconds as in a much longer

time, even more than a day. There is logically no more justification for clinging to the sun navigation hypothesis in the face of these results than there is for holding on to the kinesthetic and magnetic hypotheses.

If there is any need to qualify this statement, it would be on the ground of the *amount* of evidence against the sun navigation hypothesis rather than its quality. Kramer's method of working with virtually untrained pigeons represents a distinct advance in experimental methods. In fact, he and von St. Paul make it quite clear that they did not expect their birds to show such a clear-cut homing ability under these conditions. Both Matthews and Hitchcock have in correspondence with the writer expressed surprise and some skepticism at the excellent homing results obtained. There is therefore a need for repetition of these experiments by other investigators. If this can be done, it will serve two main purposes. First, it may confirm the finding that pigeons have a highly developed homing ability, and secondly, if Kramer's results from his comparison of the light and dark birds (as well as those of his tests of the sun's height) can be repeated, it will give further scientific assurance that the sun is not the key to the choice of the home direction. This is worth while because the sun navigation hypothesis, although only recently advanced, has within a short time been given strong emphasis and wide circulation. Matthews put it forward almost as the only remaining sensory basis for homing. If, as now seems to be the case, this suggestion is wrong, it should be completely excluded experimentally before the error gets rooted in.

There seems to be no reason to doubt that Kramer's basic findings will be confirmed. He naturally had faith in his own results, and on the basis of them offers some tentative conclusions about the homing ability of pigeons. He is unwilling for the time being to credit the birds with the very special abilities that would enable them to fix their position by observing the sun for 40 seconds. There must be some other way.

Kramer suggests that pigeons have some way of knowing where they are which is independent of the sun. This is the basis of the first stage of homing. The second stage, which begins from the moment the homeward orientation first appears and continues as long

as the birds are over strange territory, is the period during which the bird is flying in the direction that is consistent with the impression formed in the first stage. Kramer suggests that the sun is a factor in the second stage, as both Matthews and he observed that flights under overcast skies with the sun not visible involved circling and wide deviations from the home direction. The known facts are consistent with the view that pigeons keep on a chosen course by observing the position of the sun in azimuth and interpreting it appropriately for the time of day.

Theoretically, Kramer recognizes that when the sun is not visible the first stage of homing may continue all during the time the bird is in flight. In other words, by means of this unknown method of orientation the pigeon may repeatedly renew and correct its impressions of the direction of the loft. The circling, inaccurate flights observed under heavy clouds, however, suggest that once the definite direction is established by this unknown method, the flight may proceed on a straight course when the sun is visible. The sun may serve as a "skymark" by which the bird keeps its flight on the chosen course. But what is the method by which this course is selected in the first place, and by which the impression of the home direction is, possibly, renewed and corrected from time to time during the flight? This is the primary problem of homing. It is still unsolved, and the solution is closer only in the negative sense that explanations previously considered possible have now been largely excluded.

Does the Choice of the Home Direction Depend upon Sensory Stimulation?

The advanced point to which the research on the homing problem has been carried, particularly by Kramer's 1952 work, represents a forward position for a new attack upon the homing problem, one from which the investigators seem unlikely to be dislodged. Quite naturally and properly the approach favored in previous research has been to seek the explanation within the realm of sensory possibilities. The net effect of this experimental work has been to establish more clearly the existence of a true homing ability in the pigeon but at the same time to make it appear unlikely that any of the previously

suggested sensory hypotheses will adequately explain this behavior.

The first question that arises is whether homing has some sensory basis that has not yet been thought of or seriously considered. The vast majority of zoologists and other investigators of animal behavior would answer this question by saying: "Of course, if the present sensory hypotheses have to be rejected, the answer must lie in some sensory basis that has not yet been considered." For example, Hitchcock says at the end of his recent experimental report that his results can best be described by saying that the pigeons have a "sense of direction." He makes this suggestion in spite of the fact that he had already stated earlier in his article that he was not happy about hypotheses falling in this general category, and he consistently uses quotation marks about the term. The point that is significant for the question with which we are now concerned is that, in spite of the fact that he is not able to offer any suggestion regarding the nature of this ability, he nevertheless concludes with the statement: "With continued investigation, the nature of this 'sense' should be reduced to physical dimensions."

I believe that this is the prevailing scientific view. There seems to be no danger, therefore, that the sensory aspects of the problem will be neglected. But if the efforts to explain homing entirely within this framework should continue to fail, the reason could be that the boundary lines of the investigation have been arbitrarily drawn in such a way as to exclude the essential principle. By all means, investigators should continue to look for a sensory basis of homing; but it should be recognized at the same time that there is no way to define the area within which all future scientific discoveries must be made, and therefore the possibility cannot be excluded that homing may involve a kind of adaptation which does not have a sensory basis.

Why Ask Whether Homing Orientation May Have an Extrasensory Basis?

To raise and attempt to answer this question is the main purpose of this review. It has been necessary to review the present experimental situation fully before reaching this point because the question

is one that can only be answered in relation to the status of the sensory hypotheses about which the research has thus far been centered.

The question, essentially, is whether the research on the homing problem should not be broadened to include the possibility that this animal orientation has some energetic basis that transcends sensory functioning, that it depends upon essentially the same kind of abilities as those that parapsychologists have been investigating for many years in humans. I am not implying that further research will definitely prove that ESP is the basis of homing. But is the likelihood it may do so strong enough on the basis of the present facts to justify undertaking a program of research planned with this hypothesis specifically in mind? And if it should turn out that ESP is the answer, how would the resulting advance in knowledge in the area of the problems with which parapsychology is concerned compare with what might be accomplished by an equal effort in some other direction? The answers can be indicated quite briefly.

1. There is at the present time no sensory hypothesis of homing that has not already been seriously called into question by the facts as they are now known.

2. ESP has been established in humans, and this evidence provides a proper scientific basis for asking whether the same factor may be detected in animals, particularly in connection with behavior which suggests the operation of psi. For a comprehensive review of animal behavior which seems to be relevant to the question of psi in animals, see Rhine (**12**).

3. None of the research on pigeon homing done heretofore has produced evidence against the ESP hypothesis, and to the extent that the experiments have made a sensory basis of this behavior appear less likely, they have indirectly made the psi explanation more plausible.

4. If ESP is the basis of pigeon homing, it is operating at a level of efficiency and control that has rarely been approached in human subjects. On the one hand, this contrast might suggest that what the birds are demonstrating is an altogether different kind of function and therefore it is not likely to be extrasensory. But on the other hand it is not unreasonable to suppose that the psi factor may not have been limited in development (or repressed) by the process of evolution in all the lower species to the extent that it has been in man. It is conceivable that some species, such as birds and fish, whose means of locomotion is too highly developed to be at all times adequately directed by the senses may have retained the psi function as a means of orientation toward distant goals

in their evolutionary development or even had it strengthened. If this should be found to be the case, the discovery would have a revolutionary effect upon present-day biological theories. Also, parapsychology would have discovered an area of life in which its principles are much more readily available for investigation, and further research with pigeons and other species with outstanding homing ability might well have important bearings upon fundamental problems of human nature.

5. In my opinion, research on the question whether ESP is involved in the homing of pigeons should begin at the point that has now been reached by past investigations in this field. Insofar as possible, the work should be done in close cooperation with the zoologists who are approaching the problem from the sensory side. The aim of all the investigators should be—and must be if the research is to be truly scientific—to discover the facts as quickly as possible. This can be done only if every active experimenter is ready and eager to root out the erroneous hypotheses.

6. Merely disproving every conceivable sensory hypothesis, however, will not be sufficient to establish the ESP basis of homing. Before this conclusion will be justified, it will be necessary to identify the ESP basis of this behavior and to demonstrate it experimentally in terms of its positive characteristics and consequences.

Some suggestions regarding a research program for testing the ESP hypothesis of homing may be offered, though they must necessarily be tentative at this stage. What is required, in effect, is an ESP test of pigeon homing. One thinks first of the progress that has been made toward developing methods for testing ESP in other species: the work of Bechterev with dogs (**1**) and the recent investigations of Osis dealing with a psi relationship between man and the cat (**11**).

Encouraged by these examples, the pigeon investigator may be tempted to think that all he needs to do to find out whether this species has ESP ability is to design a suitable multiple-choice situation with sufficiently strong motivation and adequate controls against sensory cues. This might, however, represent a big psychological change for the pigeon, and even if ESP is involved in the usual homing situation, there is no secure basis for predicting that the birds would use it in a laboratory test.

Perhaps a more promising experimental approach is to find a situation which offers the pigeon a definite choice between two pos-

sible "home" directions but under circumstances as similar to those of previous homing tests as it is possible to obtain. Tentatively, I have been thinking of an experiment to be carried out along the following general lines. Young birds will be settled in a movable loft as soon as they can be weaned. This loft would be located on successive days at three spots, A, B, and C, the birds being allowed to live a normal life in and about the loft at a given place during one day and the loft being moved during the night to one of the other two locations for the following day. The three locations would be selected to form a triangle with sides of a suitable distance, probably 10 miles. Each location would be one in which the loft is shielded by a building or some other obstruction so that it could not be seen by birds in flight at the other two places. In fact, the arrangement should be such that pigeons released at one of the locations could not see at which of the other two the loft was situated until they had flown at least half the distance.

The procedure of moving the loft about among the three locations would be continued until the birds showed signs of being fully at "home" at each place in their exercise flights about the loft and until the birds were sufficiently mature for homing tests. They would then be accustomed to homing releases by taking them a few times a distance of three or four miles from the loft at each location and releasing them to return to it.

The final, ESP stage of the test would be reached when the birds could be taken from the loft at one location and kept in crates while the loft was moved on a random basis to one of the other two places. All necessary precautions would be taken against sensory cues, such as sounds or odors revealing where the loft had been taken. After a suitable period, someone who had stayed behind with the pigeons would release them and observe the direction of their departure flights. If the birds should show any tendency to start toward the loft rather than dividing on a chance basis between the other two locations, the findings would provide direct evidence in support of the ESP hypothesis of homing and would open up unlimited possibilities for further experimental study of the behavior.

Failure in the first attempt at such an experiment should not discourage further efforts, and especially it should not be interpreted

to mean that the ESP hypothesis of homing has been disproved. It may be that what appears to the human observer to be an adequate test situation may not be any test at all for the pigeon. For example, the distances of ten miles may prove to be completely inadequate, as it may be a matter of complete indifference to the pigeon whether he has to fly that distance or twice as far to find the loft. Of course, the search for the solution to the homing problem will end for every investigator, regardless of the point of view from which he has been approaching the question, as soon as the explanation is found. So long as the main goal has not been reached, however, the same kind of persistence and versatility may be required in testing the ESP hypothesis as has been used in seeking an explanation in sensory terms.

APPENDIX

Preliminary Report on a Homing Experiment with American Pigeons

The first requirement for a program of research on the problem of pigeon homing is to be able to obtain results comparable to those first reported by Kramer and von St. Paul of the Max-Planck-Institut, Wilhelmshaven, Germany. American investigators who have been working on this problem have consistently obtained poor homing performance from their pigeons. Indeed, until recently, many of them have doubted that pigeons have any ability not adequately explained by direction training, random searching, and the use of landmarks.

In the spring of 1952, I obtained a group of young birds for homing experiments. A shipment of 40 homing pigeons were purchased from Palmetto Pigeon Plant, Sumter, S. C., and 40 were assigned for the research by the U. S. Army Signal Corps Pigeon Center at Fort Monmouth, N. J. The young birds used in the present experiment were bred on the location from old stock provided by the Tarheel Racing Club of Salisbury, N. C., and by Mr. A. F. Phaup of Richmond.[2]

[2] I wish to express my appreciation to those who helped me to obtain pigeons, as well as to the many others, too numerous to list, who gave me invaluable advice and encouragement.

During the summer, I began training the 80 older birds by releases at increasing distances about the loft, using the methods that have been traditional with American pigeon racers and that have been adopted by other investigators. My homing results were disappointing. By the time the birds were being released at a distance of 40 miles, they were showing no evidence of homeward orientation at the release point. Approximately half of them were lost in each test at this distance and those that returned took so long to reach home that the results were quite ambiguous.

There was clearly some fundamental difference between my methods, as well as those of other American investigators, and the procedure followed at the Max-Planck-Institut. The Parapsychology Laboratory therefore invited Dr. Kramer to come to America for a period of collaboration on the pigeon homing research at Duke University. Dr. Kramer accepted, and he spent approximately two months in this country. He visited the lofts of the Parapsychology Laboratory for one day shortly after he arrived to make as early as possible any changes needed in the manner of caring for the birds. The chief change which Dr. Kramer suggested was that the birds be given complete freedom about the loft. Prior to his visit, they had been confined except when they were allowed out for exercise.

He then returned after a fortnight for a month of uninterrupted collaboration. We were able to carry out a small-scale experiment modeled as closely after the Wilhelmshaven experiments as the stock situation of the loft permitted. At that time, the only birds available which could be used without jeopardizing the breeding program and plans for future research were young birds (3-5 months). There was no basis for predicting that young birds would show definite homing behavior at this season of the year, but I urged that we go ahead nevertheless since it was the best method for me to observe the procedure at every stage of the experiment. We used 16 young birds in the experiment and one nine-months-old bird of Fort Monmouth stock.

In the preliminary, accustomization stage of the experiment, there was time for only three releases at 10 miles in different directions, the last one being from the north. We then took the pigeons

in crates, covered with tent canvas, to a lookout tower (Bishopville, S. C.) 147 airline miles SSW.

The birds were released at Bishopville in six groups, the first one being a single release of the nine-month hen. All departure flights except that of the youngest group were immediate and without circling. Five groups of the birds vanished from sight as observed with field glasses at approximately 1½ miles in from 1½ to 2½ minutes; the youngest group took 7 minutes. The vanishing points were recorded in terms of the degrees of the circle, with N = 0° (360°), E being 90°, etc. All vanishing points were between 305° and 340°, inclusive. As the bearing of the loft was 31°, all six vanishing points were within the home half of the circle. However, there was a consistency in the deviation toward the left which suggested the presence of some local factor or factors producing departures in that sector.

The homing results exceeded our expectations. The oldest bird homed in four hours. Six of the young birds arrived home on the day of release, one by the morning of the following day, and another by the morning of the second day following.

The birds carried messages requesting reports, and information was received on five of the eight birds that failed to home. Two reached Salisbury, N. C., 100 miles from the release point and 43° to the left of the home line. The other three were found not more than 55 miles from the tower and presumably they were lost, though one of the short-distance travelers was found 25 miles away on the direct line toward home. The other two went toward the west, outside the homeward half of the circle. It will be interesting to observe on the basis of reports from later experiments whether there is a tendency for long-distance flyers to end up in the direction of home while short-distance flyers are found randomly distributed about the release point.

The experiment, limited in scale as it was, provided the answers to some questions, both practical and scientific, that are basic to further research. This preliminary report of the experiment is merely for the purpose of putting the essential facts of the experiment on record as showing that American homing pigeons perform in a manner comparable with German pigeons when they are handled in the

same way. The experimenters hope to issue a full, joint report after the results have been extended through further releases.

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AN EXPLORATORY STUDY OF SOME VARIABLES RELATING TO INDIVIDUAL ESP PERFORMANCE[1]

By R. L. Van de Castle

ABSTRACT: The experiment described here was designed to test whether a single ESP subject would show variations in score level corresponding to: (1) differences in the kind of ESP target material used; and (2) differences in the length of time between the presentation of the target and the subject's response to it. The stimulus materials used were numbers, drawings, letters of the alphabet, colored cards, and ESP cards. The targets were enclosed in black envelopes and given to the subject, who picked up the top card, waited for a designated time, made her call, laid the card aside, and went on to the next. The time intervals were varied as follows: 0-2 seconds, 3-10 seconds, 11-30 seconds, 31-60 seconds, and 61 seconds or more.

The 65 runs that were carried out gave a positive deviation of 64 hits, a statistically significant number (CR = 3.97). Two kinds of targets, colors and ESP symbols, gave scores that were independently significant. There was a fairly close relation between score level and time interval—the longer the interval, the higher the scores—and the three longer intervals were independently significant. The variation in results among all five types of target material, as well as among the five different time intervals, was more than would be expected in any one of a hundred such series in which only chance variation occurred. The author discusses various interpretations of these differences.

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Introduction

A great deal of experimental work in the field of parapsychology has been undertaken in an effort to discover the relationships that exist between the physical variables of an ESP testing situation and the resulting level of success in scoring. In keeping with this trend, experimenters have used a wide range of stimulus objects in ESP tests, such as playing cards, color cards, animal cards, ESP cards, numbers, drawings, proper names, and even sounds. Many of these stimuli have in turn been subject to still further investigation as in

[1] The author wishes to express his appreciation to Dr. J. B. Rhine for his original suggestions concerning the design of this study and for the advice and encouragement he graciously offered throughout the entire study.

the case of ESP symbols when they were distorted (3), changed in size (4), and varied with respect to the number of them on the individual card (5).

This type of experiment can be traced to a rationale that is involved in the projective theory in psychometrics, that is, that perception (or in this case extrasensory perception) is a function of both the individual perceiver and of the stimuli which elicit his behavior. Thus it seems to follow that scoring rate in ESP tests should be affected by the nature of the targets used. However, treated as a whole, the results tend to show that none of the alterations in the ESP cards produced any significant changes in scoring and that other types of stimuli were about equally effective in producing scores indicative of ESP.

Although at first glance it appears that these results do not support a theory that the nature of the stimuli is a differentiating factor in ESP scoring, the author felt that such an interpretation might have been due to ignoring the other and more basic tenet of the projective theory—that perception (or ESP) is also a function of the individual perceiver. Any individual change in the level of scoring associated with a difference in stimuli might be obscured and overshadowed when the results for a group of subjects were considered as a whole, as has often been the case in the past. In order to test this assumption it would be necessary to examine a *single* individual's pattern of ESP results when presented with a variety of stimuli.

There is good reason also for considering that the level of scoring success in ESP may be a function of the individual's rate or speed of perception. This would be similar to the concept of *personal tempo* introduced by Stern in 1900 (12) which he later described by saying, "Each individual has his personal tempo, that is, an optimal time of rhythmic events in life and experience"(11). Some evidence for such a hypothesis as applicable to ESP exists as the result of Stuart's experiment (13) in which he found that there was a significant difference in scoring levels between preferred and non-preferred speeds in responding to metronome rates on a test of blind and screened touch matching. But a later investigation by Stuart and Smith (14) did not confirm this result because, as they said, "The significant effect of uncontrolled variables, supposedly of a motivational type,

indicates that the matching rate is not alone an important enough factor necessarily to produce differential ESP scoring." However, both of these results were concluded on the basis of scores from a group, and therefore emphasis was not placed upon individual variations in such a method.

From the preceding review, it can be seen that although many attempts have been made to investigate the role of stimulus variation and response variation in tests of ESP, the methods employed have usually failed to take into account what effect these variables may have upon the scoring level of any particular individual. This situation seemed worthy of further consideration and experimentation, and consequently it posed the problem for research upon which this paper is based.

The Problem

The purpose of this study was to discover in relation to a single subject:

1. Whether statistically significant scores under conditions justifying an ESP interpretation were present.
2. Whether different stimulus materials would result in different levels of scoring.
3. Whether waiting for different time intervals to elapse before responding to these stimulus materials would result in different levels of scoring.

Subject and Materials

The subject for this study was my wife, Doris, 27 years old. She is interested in parapsychology and obtained a few scores well above chance during some informal testing preceding this study. Doris is also an excellent hypnotic subject. Her mother occasionally reads a teacup for a friend and is said to have had some success with her predictions.

The stimulus groups used in this study were:

1. Regular ESP cards (star, cross, square, wavy lines, circle).
2. Numbers (one, two, three, four, five).
3. Drawings (face, book, tweezers, flower, cigarette).
4. Letters of the alphabet (a, e, i, o, z).
5. Colored cards (red, green, brown, yellow, blue).

The numbers were drawn in ink on small cut-up sections of bibliography cards. The drawings were made in black pencil by the author on small pieces of notebook paper. The letters of the alphabet were made in colored pencil on this same type of paper. The colored cards were cut-up sections of regular paper.

All stimulus cards within a group were cut the same size, and all were enclosed in black opaque envelopes obtained from the Duke Parapsychology Laboratory.

Procedure

For each of the five stimulus groups there were 50 cards, 10 of each symbol. These were shuffled and cut several times, and the first 25 cards were chosen and inserted in opaque envelopes out of the subject's sight. The unsealed envelopes with the cards enclosed were then shuffled and cut again before being presented to the subject. Thus neither subject nor experimenter was aware of the correct order of the cards. The stimulus group (such as drawings or numbers, for instance) which was to be used for each run was arbitrarily chosen by the experimenter, except that he checked over the records before the study was completed and arranged to have each group selected an equal number of times.

It was decided that five different time intervals would be used for the subject's responses. These time intervals were: 0-2 seconds, 3-10 seconds, 11-30 seconds, 31-60 seconds, and for as long above 61 seconds as the subject desired. During each run of 25 cards, all five time intervals would be utilized, so that each segment of five consecutive cards would be called according to a different tempo. The order of these time intervals was also arbitrarily decided by the experimenter, with precautions taken to insure that each segment of five cards within the run would be responded to an equal number of times for each of the five tempos. Thus segment one (first five cards) was responded to with a time interval of 3-10 seconds as many times (thirteen) as it was with a time interval of 31-60 seconds, etc.

Because of the household duties, it was impossible for the testing to be carried out in a regular laboratory, so all the runs were performed at our home. My wife would usually sit on a couch with a

deck of stimulus cards by her right side. As she picked up each envelope, she called out her guess and laid the envelope containing the card she had been holding to her left side, being careful to place it so as to form a neat, orderly pile. I recorded her calls from a few feet away on a regular ESP sheet and then recorded the correct order later as the subject reported it to me from the left-hand pile after the run was completed. During some runs I observed each card as she took it from the envelope but did not always do so. The subject could not see her calls as she reported the cards.

The detailed procedure for one run was as follows: The experimenter selected some stimulus group, say drawings, and prepared the deck as described before. He then handed it to the subject and told her which group she was using. The experimenter also privately listed the order in which the five time intervals would appear and informed the subject that the first five cards would be called at, say, a 3-10 second tempo. The subject picked up an enclosed card from the top of the deck and held it lightly between her fingers. When three seconds had elapsed, the experimenter said "Begin" and some time between then and seven seconds later, the subject made her guess, which the experimenter recorded. This continued until five cards were called and then the subject was informed which interval would come next, say 31-60 seconds. The subject picked up the next card, held it for an "incubation period" of 31 seconds and was then told to begin. Sometime after this signal was given and before 29 seconds had elapsed, her call was made and recorded and the card was placed on the left-hand pile. Thus it can be seen that each time interval consisted of an "incubation" period and a "concentration" period. When five calls had been made at this 31-60 second interval a different tempo was introduced. This procedure continued until all five intervals had been responded to, after which the check-up of hits was made.

This method of going through a run of 25 trials took 20-25 minutes. Only one run was done at each session, which was in the evening whenever the subject could spare the time. The experiment was continued for 65 runs or 1,625 trials during a six-month period, this length of the series having been set before the experiment was started. There were 13 runs of each of the five stimulus materials,

or 325 trials apiece, and there were also 325 trials for each of the five time intervals (5 trials for each of 65 runs).

Results and Evaluation

The 65 runs which were performed during this study yielded a positive deviation of 64 hits. The standard deviation for this number of runs is 16.12, giving a CR of 3.97 for the total number of hits made in the experiment. This is a result that would be expected on the basis of random sampling less than once in ten thousand times.

Table 1 shows a breakdown of these hits into the five target stimuli. The colored cards gave the largest number of hits while drawings produced the lowest. Only two kinds of targets, the colors and the ESP symbols, were independently significant, the former at the .000,003 probability level and the latter with a P-value of .013. To evaluate the variation in results among the five kinds of targets, a chi-square test was applied to the two-by-five contingency table of hits and misses in the five stimulus groups. The value of χ^2 was found to be 13.31 (4 degrees of freedom), which indicates a significant degree of variation, one that would be equalled or exceeded by random sampling an average of only one time in a hundred.

Table 1

Distribution of Hits According to Type of Target Stimulus

Stimulus	Runs	Hits	Dev.	CR	P
Color	13	99	+34	4.71	.000,003
ESP cards	13	83	+18	2.49	.013
Numbers	13	74	+ 9	1.25	.2
Letters	13	71	+ 6	.83	.4
Drawings	13	62	− 3	−.42	.7
Total	65	389	+64	3.97	.000,072

Table 2 shows the distribution of hits when the data are grouped according to the length of the time interval that the subject waited prior to making her call. The 31-60 second group gave the highest number of hits, closely followed by the 61-second-or-longer interval.

Table 2

DISTRIBUTION OF HITS ACCORDING TO LENGTH OF TIME INTERVAL

Time Interval	Runs	Hits	Dev.	CR	P
0-2 sec.....	13	63	− 2	− .28	.2
3-10 sec.....	13	66	+ 1	.14	.9
11-30 sec.....	13	82	+17	2.35	.02
31-60 sec.....	13	90	+25	3.46	.0006
61 sec. and up...	13	88	+23	3.19	.0014
Total.....	65	389	+64	3.97	.000072

Responding immediately (0-2 seconds) gave the lowest score, the 3-10 group was not appreciably higher and the intermediate interval of 11-30 seconds gave results that were nearest the over-all average. There was, therefore, a fairly close relation between the score level and the length of response interval. A chi-square test of the variation in results applied to the two-by-five table of hits and misses, as described in the preceding paragraph, gives a χ^2 of 10.63 (4 d.f.), with P being less than .04. However, when a regression test is made which takes into account the relation between the scores and the five intervals arranged in terms of increasing time, the coefficient of linear regression is found to have a value of $t = 4.53$ (3 d.f.), with P = .01. This result cannot be considered conclusive, as a linear relation was not explicitly predicted; but it certainly justifies attention to this point in future tests to see if the finding may be confirmed.

DISCUSSION

These results, on the basis of their statistical analysis, seem to offer evidence that ESP was functioning and was utilized by the subject in obtaining her extrachance results. However, before such an assumption can be made, other alternative hypotheses must be first examined.

To start with, was there anything about the materials themselves that would enable ordinary sensory processes to be operative to the extent of obtaining extrachance scores? The materials were enclosed, but not sealed, in black opaque envelopes. The cards were all small enough to fit inside the envelopes so that no portion pro-

jected into view. There were no raised or indented surfaces on the cards which would afford tactual clues through the envelopes. It was noticed after the completion of the study that there were very slight differences in texture in the colored slips of poster paper when felt by the bare hand. However, these differences could not be detected through the envelope by either the subject or the experimenter. In any case, the subject never rubbed the envelopes or handled them extensively, preferring instead merely to hold them at the corner between her thumb and forefinger with very little tactual pressure. It was not felt therefore that the difference in textures was related to the fact that the highest score was made on the colors.

Another counterhypothesis deals with possible recording errors as an explanation of the results. Since there was no independent recording of the cards and calls, I took special pains to avoid errors in making the records. We were also alert to the possibility that the subject might make errors in calling out the cards as she removed them from the envelope. The targets were recorded in reverse order, and the kind of error with which we were chiefly concerned was that the subject might remember her call for a particular trial and unconsciously make a motivated mistake in announcing the target to get a hit. This was obviously most likely to happen for the last few calls, which were checked first, so the experimenter observed those targets as the subject took them from the envelopes. As stated previously, the subject could not see the call record as she announced the target order. The best evidence that motivated errors did not influence the result is in the fact that significant results were obtained in terms of the differences both among kinds of stimuli and among lengths of response intervals. The differences that occurred were not predicted nor expected and the fact that they were present to a significant degree was not known until the analyses were made at the end of the experiment.

Next, was there anything in the design of the experiment itself which might be conducive to an alternative explanation? The only criticism that occurs to the writer is that the randomness of the card order might be questioned. This is valid to the extent that only 50 cards of each group (10 of each specific stimulus) were used, and it was impossible for more than 10 cards of any symbol to appear in a

run of 25. This would not, however, represent any appreciable departure from the theoretical variance for random targets, as the correct variance for runs done in this way would lie between 4, the theoretical value, and 4.16, the value for the regular five-by-five ESP deck (2). The expectation would not be affected.

It can be seen that this study was not conducted with rigid "lock and key" laboratory conditions and controls. Nevertheless, its informality was not such as to exclude it from serious consideration, and its value as an exploratory study is enhanced by the variations in scoring in relation to the different conditions.

Let us now turn to the problem of interpreting the different scoring levels associated with the differences in stimulus targets. Is there anything about colors or drawings *per se* that should cause them to be on the opposite poles of scoring success in this study? The answer seems to be no, judging by the many reports of successful work with drawings. Also, at least one study using colors (7) failed to produce any significant results. As was mentioned in the introduction, a wide variety of stimulus targets have been used in ESP research and all of these have produced extrachance scores. The superiority of one type over another is discounted by most investigators; for example, Carpenter and Phalen were led to conclude: "For a subject who can guess cards with scores beyond chance expectancy, color stimuli are as effective as form stimuli" (1). It would appear, therefore, that the discrepancies in scoring level observed in this experiment must be traceable to some differential factor existing within the subject.

There are two hypotheses regarding the differential scoring levels that deserve consideration. One is that advanced by L. E. Rhine when she makes reference to the immense part that suggestion can play in producing results (5). She says: "If a subject be prejudiced beforehand against one type of stimulus, it is entirely possible for him to fail in ESP tests with that particular stimulus; another subject, not prejudiced, might have no difficulty with the same stimulus." Thus, although the subject reported no conscious preference or prejudice for any of the target stimuli at the onset of the study, and although she did not consciously keep track of her scores with the different stimuli as the experiment progressed, it is difficult to rule

out the possibility that she was unconsciously influenced by her results and developed preferences which affected her scoring.

A checkup of the first three runs of each stimulus group revealed that already at that early stage color was in first place with 22 hits, followed by ESP cards and numbers, tied for second and third place with 18 hits each, then the letters of the alphabet with 15 hits, and lastly drawings with only 11 hits. Exactly the same positions were held at the end of six runs for each group. It appears, therefore, that the differences among the stimulus groups were consistent from the beginning of the tests and that they did not change as the tests progressed. On the suggestion hypothesis this would mean either that preferences existed on the basis of some unrecognized factors present before the experiment started or that the subject seized upon the chance variation of the first few runs as setting the pattern to be followed throughout the tests.

The other theory is the one advanced by M. Skibinsky who says: "It may be that target material which acts as a stimulus to the production of personal associations within the individual being tested tends to cause negative scoring in tests" (8). Skibinsky designed an experiment to test the hypothesis that subjects will score higher on ESP targets which are psychologically very meaningful than on targets which have little or no personal significance.

Actually the drawings we used in our study were all selected because they held some personal association for the subject. We had thought that meaningful material of this type would prove to be better than the cut-and-dried material, such as the ESP symbols, ordinarily used. Skibinsky, in testing his hypothesis, used family names and compared the scoring level obtained with them to that obtained from ESP cards. He was surprised when his results ran counter to his expectations. I was not familiar with his article until after this study was completed. My results tend to confirm his, therefore, even to the point of having been unexpected.

If this interpretation is correct, the next lowest score should be obtained on the next most meaningful level of stimulus material. Although such ranking of the target material is difficult, it does seem probable that letters of the alphabet would be richer in personal associations than either color or ESP cards. Even saying such letters

as "I" or "U" would seem to be a logical point of origin for a host of associations. Therefore, although it cannot be ascertained whether the empirical rankings for target stimuli resulting from this study would actually correlate with subjective rankings of meaningfulness or significance, it does appear tenable, especially in view of Skibinsky's earlier findings, that the association value of a stimulus may be inversely related to its effectiveness as an ESP target. (This aspect is being investigated by the author in another study now under way.)

Turning now to the subject of time intervals and their relationship to ESP performance, the question arises as to whether a certain time interval *per se* produces significant extrachance scores. Once more the answer is no, for Riess's famous high-scoring subject made her guesses at about one-minute intervals (**6**), while another famous high-scoring subject, Mrs. Stewart, took less than a minute to call an entire deck of 25 cards (**10**). The important factor appears to be whatever rate is best for the individual subject.

At first glance, it seems reasonable to compare the present results with those on the effect of tempo reported previously by Stuart (**13**) and Stuart and Smith (**14**). However, it seems doubtful if imposed waiting periods of the length of the ones used here are to be compared, psychologically, with the relatively slight slowing down or speeding up of the subject's response rate in those studies. The longer, more effective intervals of the present tests were actually "waiting" periods. Since no introspective records were kept of the "incubation period" before the subject responded with her call, it is practically impossible to isolate what the causative factor might be. Sometimes she talked, smoked, listened to the radio, or even read during the longer incubation periods, while only casually trying to secure an impression of the card. It was not until after the "begin" signal was given by the experimenter that the subject consciously concentrated upon the card. Questions that can be answered only by further work are whether this subject would continue to score better with the longer intervals in further tests and to what extent this effect applies to other subjects.

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (—1), (—2), (—3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

ON THE RESISTANCE TO NEW CONCEPTS

On returning now to our argument from the evolutionary processes governing the growth of science, the significance of the bitter struggles that new theories may create within science can be appreciated more fully. The fitness of new knowledge, like the fitness of a species, is most convincingly demonstrated and most advantageously molded by vigorous contest. The situation outside the sciences is not so very different; predominant religious and social concepts have not developed quickly or conquered quietly. In order to change the direction of development in a field of learning, people's minds must be changed. Even in science this is a slow process, sometimes an impossible one. Max Planck said, with perhaps only a little too much bitterness about his own early struggles: "An important scientific innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul. What does happen is that its opponents gradually die out, and that the growing generation is familiarized with the ideas from the beginning." And as if to prove his point, it was Planck himself who, five years after Einstein's first publication on the photon theory of light, angrily commented that all the fruits of Maxwell's great work would be lost by accepting a quantization of energy in the wavefront "for the sake of a few still rather dubious speculations."

—Gerald Holton, "Duality and Growth of Physical Science"
American Scientist, January, 1953.